PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCIV.

PART I.

LONDON,

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

ERRATA.

Page 14, line 17, *for* XY, *read* XI.

Page 151, line 4, *for* remaining semiarc, *read* remaining part of the semiarc.

Page 153, note, line 6 from the bottom, *for* 91, *read* 87.

Ibid. note, line 4 from the bottom, *for* 5, *read* 9.

Page 167, line 14, *for* so adjusted, *read* adjusted so.

PHILOSOPHICAL TRANSACTIONS.

I. *An Account of the Discovery of a Comet. In a Letter from Miss* Caroline Herschel, *to* Joseph Planta, *Esq. Sec. R. S.*

Read November 7, 1793.

SIR,

LAST night I discovered a comet near 1st (δ) Ophiuchi, but clouds covering the part of the heavens where it was, its place could not be obtained.

My brother has just now (7 o'clock) determined its situation, as follows.

The comet precedes the 1st (δ) Ophiuchi 6′ 34″ in time, and is 1° 25′ more north than that star.

I remain, Sir, &c.

CAR. HERSCHEL.

Slough,
Tuesday, Oct. 8, 1793.

II. *Account of a new Pendulum.* *By* George Fordyce, *M. D. F. R. S.; being the* Bakerian *Lecture.*

Read November 7, 1793.

Let AB and CD be two rods of any solid of the same species, and of a simple or uniform texture. Let these two rods be exactly of the same length; let them be connected at the top with a rod BC, which is perfectly inflexible, let the angles ABC, and DCB, be both right angles, so that AB and DC shall be parallel to each other, and in the same plane; let the rod AB be fixed at the point A, and perpendicular to the horizon: then the rod CD shall likewise be perpendicular to it, excepting for the curvature of the earth between B and C, which in a foot or two may be considered as nothing: let the rod CD be loose at the end D, so as to be capable of rising up or falling down; in this case, if heat be applied equally to both rods, so as to expand them both, and lengthen them, the rod AB will raise up the rod BC, and lift up the rod DC; but the rod DC being equally lengthened by heat with the rod BA, the point D will be brought downwards by the lengthening of the rod DC, as much as the point C is raised by the lengthening of the rod AB by the heat. In consequence, the rod DC will have its end D in a line exactly parallel to the horizon, and cutting the end of the rod AB at A, as it did before the heat was applied; and the same thing will be true if the rods AB, DC, be shortened by exposure to cold; so that in all cases of heat or cold the end of the rod CD at D, and the end of the rod BA at A, shall be in a line parallel to the horizon.

Take a point E near any part of the rod CD, and let that point E be connected with the point A, where the end A of the rod AB is fixed, and let the matter which unites them be perfectly inflexible, and incapable of being altered by heat; then, I say, that the part of the rod CD, intercepted at the point E, and forming ED, will always be of the same length whether the temperature of heat is greater or less. For supposing a point F be taken near the rod AB, and of the same perpendicular height with E, so that a line drawn from E to F shall be parallel to the horizon; if the part of the rod AB, opposite to F, should rise up in consequence of being expanded by heat above F, it will carry up the point of the rod DC, which was opposite to E, to an equal height with itself, and therefore would cut off from the length of that part of the rod which was formerly opposite to E, a length equal to that which was added to what was formerly ED, by the heat. Therefore the point opposite to E, in the rod DC, will form ED, which will always continue of the same length, if the heat be increased; and by similar reasoning, it will likewise continue of the same length if the part of the rod AB AF is shortened by cold; therefore the part of the rod DC cut off by the point E, so as to form ED, will always be of an equal length, and the point D will always be of an equal height.

At the point E let there be an apparatus which will render that part of the rod DC which is opposite to the point E flexible, whatever part of it shall be opposite to the point E. Then the part of the rod DC, cut off at the point E, and forming DE, may become a pendulum. Thus we shall procure a pendulum of the same length, whatever be the degree of heat.

Let the rod AB and the rod DC be of different species of matter, so that the rod AB shall be lengthened by being heated to the same degree, more than the rod DC; then, if they be both of the same length, heat would carry up the end of the rod CD, at D, higher than the fixed point A; but if a part be cut off from AB at G, so that the whole of the expansion of the remaining part GB, shall be equal to the whole of the expansion of the whole rod DC, and that in every increase of heat, then the same thing would happen; and the part of the rod DC, cut off by the apparatus at E, would always remain of the same length. If, therefore, it is wished to render DE always equal in length, the fixed point A must be brought nearer to B, so as to shorten the rod AB, that is at G, so that the whole of the expansion of GB by heat, shall be equal to the whole of the expansion of DC by the same degree of heat.

Hitherto I have supposed that the substance which connected the points A and E was incapable of being expanded or contracted by heat: but no such substance is to be found.

I shall now suppose that the substance which connects the points A and E is capable of being expanded by heat. If it was capable of expansion equal to the matter of which the rods AB and CD consist, then it is evident that no advantage could be gained so as to render the part of the rod CD opposite to the point E down to D always equal. But it is clear that the expansion of AE, supposing the point A a fixed one, would carry the point E higher up towards C, if the heat was greater, so that ED would by this means be rendered longer; and the contraction of AE, when exposed to a greater degree of cold, would bring down the point E so as to render ED shorter, just as much as the expansion of AB would raise up the rod ED, or as its contraction lower it. But if the materials connecting the points A and E were less expansile and contractile by heat and cold, than the matter of the rods AB and CD, then upon the whole expanding, although the point E would be raised higher towards C, yet it would not be raised so high as the expansion of AB would raise the point C, and the whole rod CD. The same is true if the whole of them contract, but in an opposite direction. That is to say, the point E would not descend so far towards D, as the point C would descend towards D, and with it the whole rod CD; by this means, although the part ED would not be always equal, yet it will be much more equal than if the point C were a fixed point, and not capable of being affected with the contraction or expansion of the rod AB.

If then the part of the rod CD, from opposite to the point E to D was a pendulum, it would not be always of the same length, but it would be more nearly so than a simple pendulum made of the same materials of which the rod CD consists; and it would be nearly of the same length as if the pendulum had been made of the materials which connect A and E.

In order then to render ED always of an equal length, some other principle must be employed. Now let BA and CD consist of the same materials; and the matter connecting A and E consist of a substance that expands by heat, and contracts by cold, less than the materials of which AB and CD are formed; if the rod AB be brought down to H, and the fixed point be at H, and the points HE be connected together by the same materials which formerly connected the points AE in the rod EH, take a point K, equal in height with the points A D, and in the line AD which is parallel to the horizon, as it has already been taken in the construction; then the rod AH shall expand, upon being heated, in perpendicular height more than the rod HK, and therefore the expansion of AH shall carry the point B, and in consequence the point C, higher than the expansion of the rod HK shall carry the point E; but if the expansion of the rod AH be as great as the expansion of the whole rod HE, then the point C will be carried as much higher than the point E, as the point E is carried higher than it was before the heat was applied, and therefore the point E shall be at as great a distance from the point C, as it would have been if the materials connecting AE had been incapable of being altered by heat: and therefore if ED be a pendulum, it will be rendered of the same length. If then the rods AB, and CD, be of the same materials, and the substance connecting A and E is capable of expanding and contracting less by heat than the matter of the rods AB and CD, then, by adding to the rod AB, a part AH, under the circumstances already described, and making the fixed point at H, we can obtain a pendulum always of equal length; or if the materials of which the rod AB consists be capable of being expanded by heat as much as the materials of which CD consists, together with the expansion of the materials that connect A and E, in that case likewise the point C shall be carried as much higher than the point E, as it would be when they are expanded by heat, as if the materials connecting A and E had not been affected by heat at all. Or, lastly, if we take a rod GB, of materials which expand much more than the expansion of the matter of the rod CD, and the connecting matter of the rod EG by heat, then likewise upon the rod GB expanding, it will carry the point B, and consequently the point C, as much higher than the point E, as it would have been if the materials connecting G and E had been incapable of being expanded by heat, and therefore ED will always continue of the same length. The same reasoning will hold in cases of contraction from cold.

Therefore, if materials be employed in the rod GB, which contract considerably more than those which compose the rod CD, then the fixed point G is to be taken at a distance from B, in an inverse ratio to the inferior expansile power of the materials of which the rod CD consists, and in a direct ratio of the expansile power of the materials which connect E and G. That is, supposing that it was taken in the inverse ratio of the inferior expansile power of CD, then it would be at G; but to counteract the expansile power of the materials GE, it must be somewhat lower at I.

If then the proportion of the expansion of the materials of the rod AB, or GB, the rod CD and the materials which connected AE or GE were known, and the length of a pendulum swinging any proportion of time, in that case the distance and perpendicular height between GE and IE might be taken at once, and a pendulum might be always made which would always be of the same length, therefore swing equal arches in equal times. But these not being perfectly known, and it being extremely difficult, if not impossible, to measure off length perfectly, it is necessary to have the power of varying the distances and perpendicular height between I and E, A and E, or H and E, so that it may be found from trial whether these fixed points I, A, or H, be properly taken. For if, on constructing a pendulum on these principles, either of the fixed points, according to the circumstances, I A or H be placed too high or too low, then the pendulum will vary in its length, and of consequence swing different times in different degrees of heat.

That is to say, suppose the case be taken where the rod BG is made of materials which expand more than CD, if instead of rendering I the fixed point, it is made G, nearer to B, then the point C will not be raised sufficiently above the point E, the pendulum will become longer by heat, and make fewer vibrations in a given time. But if the fixed point in this case be brought lower than I, to the point L, then the point C will be raised higher when the whole is expanded by heat from the point E, ED will be rendered shorter, more vibrations will be performed in a given time, and *e contra*. Therefore if there be a power in the apparatus of altering the fixed point I, if found too high or too low by experiment, we shall be able to find out a true point, and make an adjustment accordingly. That is, if heat occasions a pendulum to make fewer vibrations, the fixed point is too high; if, on the contrary, it makes too many vibrations, then the fixed point will be too low.

I come now to show how these principles may be applied in structure.

As it is more convenient in practice to have the fixed point higher than A, which is equal in height to D the bottom of the pendulum, a substance should be chosen for the rod AB which expands and contracts more by heat and cold than the matter of which the rod CD consists, so that the fixed point should be at G, if the materials connecting A and E were incapable of being expanded or contracted by heat or cold:

but if their expansion and contraction is to be compensated for, the fixed point will be at I, as has already been shewn.

If the expansion and contraction of the rod IB by heat and cold, in proportion to the expansion and contraction of CD, were known, and the expansion and contraction in perpendicular height of IE, then the length IB should be to the length DC, as the contraction of the materials of CD is to the contraction of the materials of which IB is constructed, added to the contraction in perpendicular height of the materials of which IE is constructed. These lengths, in this case, might be taken at once; but it is much more convenient to have the power of fixing them by experiment, after taking them from measure as nearly as may be.

Therefore, to have a means of raising the point I higher or lower in proportion to ED, whose ends are always to be at the same distance from one another, fig. 2. represents the means of altering the point I in proportion in height to E. The frame AAAA is fixed to the substance which connects the points I and E by any means. In the clock which I have constructed, the material which connects I and E is wood, as will be described afterwards. The frame is brass; it is fixed to the wood by the screws B B, and two other opposite ones which are hid in the drawing. They are so fixed that the point I shall be nearly in the middle between the upper screw B, and the lower screw B, and so that the part of the frame DDDD shall project backwards, and carry the part of the frame EEEE behind the frame which forms the connection between I and E, so as to be parallel to it; of consequence perpendicular to the horizon. In this frame there is seen, at the upper part, in the drawing, a dove-tail groove, in which, on both sides, another frame

GGGG slides, in the same manner as in that part of a transit instrument which renders one end of the axis higher or lower. This second frame is brought higher or lower by the screw HH. The rod IB, fig. 1, is a brass tube, represented by II. There is a semicylindric cavity in the frame GGGG of the same diameter with the brass tube; a piece is applied over this frame, which is likewise a semicircular cavity, forming with the other cavity a whole cylinder, equal in diameter to the tube I I. This piece is attached to the frame GGGG by the screws L L; when these are screwed in tight, they occasion the tube I I to be embraced perfectly by the piece KK, and the semicylindric cavity in the frame G, so that the screw HH could not act if the tube I I was fixed; but if the screws L L are loosened, then they would leave the frame GGGG capable of being heightened or lowered by the screw HH. Now, if I I represent the rod IB, fig. 1, then if the centre of the screws L L be in an horizontal line, that line will cut the point I in an horizontal direction. The centres of these screws, therefore, may be considered as the point I in perpendicular height; wherefore if the point B, fig. 1, should become a fixed point, the point I may be lowered or heightened by bringing up or carrying down the centres of the screws L L by the screw HH.

If it be wished, therefore, by this apparatus to raise or depress the point I, fig. 1, it is necessary during the time of altering its height by the screw HH, that B, fig. 1, should become a fixed point; but that at all other times it should be a part perfectly free in its motion upwards or downwards.

In order to fix the point B fig. 1, in fig. 3 AAAA represents the frame of the clock, BB another frame fixed to this frame, and projecting backwards to such a distance as to carry a tube fixed to it CC so far back, and in such direction, as to exactly coincide on its inside with the cylinder formed by the semicircular cavity in the frame GG, fig. 2, and the cylindric part of the piece KK, so that if they were continued they would form the same cylinder when the screws L L, fig. 2, are screwed home. This tube is made exactly to fit the tube I I, in the same manner that the outer brass tube of a telescope is fitted to the inner one. This tube CC is split open on one side, and is of such a degree of elasticity as to open when left to itself at this slit, so as to leave the tube I I entirely free. To the frame B is fitted a piece D, which consists of two semicylinders, connected together by one being screwed to the piece B at E, the other to the frame B at E on the other side. They are also connected by a part F, where they can be brought together or separated by the screw G. Now if the screw G be undone, then the two semicylinders will separate from one another, and allow the tube CC to open by its elasticity, and the tube I I to be at perfect liberty in expanding upwards or downwards. But if the screw G be screwed home, then the two sides of the piece F will embrace the tube C, and shut it upon the tube I, so that it shall become fixed at this point. This apparatus may be placed any where between B and I, fig. 1. In the clock I have constructed, it is placed at the point F opposite to E, fig. 1.

If the point I be placed then too high, the clock will be found to go too slow if the heat be greater, too fast if it be less. If this should be found the case, then the screw G, fig. 3, is to be screwed home, the screws L L, fig. 2, are to be loosened, and the frame GGGG must be screwed downward by the screw H, which will carry along with it the piece KK, therefore the centres of the screws L L which form the point I, fig. 1, in perpendicular height. Then the screws L L, fig. 2, are to be screwed home, the screw G, fig. 3, is to be loosened so as to leave the tube I I, which is the same in both figures, perfectly free and untouched by any of the apparatus excepting at the point I, the fixed point. The same thing is to be done, only in the reverse, if the clock be found to go too fast in a greater degree of heat, too slow in a greater degree of cold.

In the clock which I have constructed, the bar BC, fig. 1, is considered as inflexible. If it were flexible, a weight hung at C would occasion it to fall lower, and in consequence bring the rod CD lower, and a greater part of it below the point E; thus the pendulum would be lengthened. It is therefore necessary to make this rod as inflexible as possible; for this reason it is made of a strong brass bar, HK, fig. 3, which is farther strengthened by a brass bar LK, supported by the upright HL.

If a weight were hung at K, it would tend to draw the tube I out of the perpendicular towards K. To prevent this, another bar HM is carried exactly opposite, so that M H K shall form the same bar, the under surface of which is parallel to the horizon. At the end of this bar M, is appended a weight O, so that, supposing the line LH to be the axis of the tube I I continued, and perpendicular to the horizon, then the whole of the weight on the side of LH towards M, shall be equal to the whole of the weight on the side of LH towards P. P is the head of a screw, to the centre of which the rod CD, fig. 1, is attached; this screw passes through the end of the bar HP, in the same manner as the wire sustaining the weight O passes through the end of the bar HM at N. The uses of this screw are to be seen in fig. 4, where HK represent the end of the bar HK, fig. 3, through which the screw A passes. The head of this screw P is toothed and divided. To the teeth there is a catch at K, which allows it to be turned backwards and forwards with a tolerable force, but keeps it in the same place until that force be applied to it. The upper part of the screw C, fig. 4, is a cylinder, which passes through a cylindric hole in the end of the bar HK, fitted to it so as to allow of being turned round without shaking. The screw itself passes through a female screw at D in the frame EF.

This frame, as well as the screw itself, is made of brass, and consists of a bar EF, which is perpendicular to the horizon; from which arise two pieces GG and EL, at right angles, therefore parallel to the horizon. In the upper piece EL is a female screw, through which the screw A passes; so that the head P, being turned round, will raise the frame EF or depress it. In the bottom part GF there is a slit MM, through which the rod NO passes. This rod passes through a piece of brass PP at Q, where it is fixed, so that by screwing forward the screw A, the point Q, and the rod NO consequently, will be raised up, and *e contra*. The point Q is placed exactly under the centre of the screw A.

This point Q is to be considered as the point C, fig. 1. For although in fact the part of the rod HK where the screw A passes, and rests upon the end of the rod HK, be the point C, fig. 1; yet if a part of the tube I I, fig. 3, at the point C be taken at an equal height with Q, fig. 4, as they are both of brass, both will expand equally. If the whole should be heated, the expansion of CH, fig. 3, will raise up the bar HK, and the expansion of the screw A, and the frame EF, will carry down the point Q, so that it shall always be at an equal height with the point C, fig. 3. The points C and Q, fig. 3, 4, therefore, being always of the same height, may be considered as the points B C, fig. 1. The rod CD, fig. 1, is steel wire, the whole length of which weighs 13 grains. It is watch pendulum wire, No. 3. The point E is at R, fig. 4.

SSSS, fig. 4, is the frame which connects the points I and E, fig. 1; to this frame the square brass bar I I is firmly fixed, so as to rise perpendicular from the frame SSSS; on the side VV of this bar a semicylindric piece of crystal is attached, so that the cylindric part shall be towards R, and so that a number of lines perpendicular to the horizon, tangents to this cylinder, shall pass through the slit MM.

WX is another square rod, with a crystal semicylinder also attached to the frame SSSS, so as to rise perpendicular to it, but sliding in the dove-tail groove XY so as that it is capable of being brought nearer or farther from the cylinder attached to the rod I I. This may be done by a screw. It is clear then that the wire NO will pass between the cylindric parts of these two cylinders.

If the screw Y was screwed so as to make the cylinders touch one another, all but the exact thickness of the wire NO, then it is clear that the under part of the wire, from V to O, would become flexible at V, but then the expansion of the tube I I, fig. 3, would not lower or raise the part of the wire VO so as to keep it always of an equal length; but if the cylinders were opened the least farther, then the expansion or contraction of the tube I I, fig. 3, would raise or depress the part of the wire between the cylinder, and render the pendulum always of the same length, whatever be the heat; and thus a pendulum may be formed, according to what has been demonstrated, always of one length.

On considering the several different methods of finding a measure of lengths which could be always and universally ascertained, I am persuaded that the taking the difference of the length of two pendulums, vibrating different times, appears not only to be the most perfect, but the easiest attainable. Mr. WHITEHURST contrived an apparatus for the purpose of ascertaining this difference, an account of which was read in the Royal Society, and afterwards withdrawn and published by the author himself. After his death, I purchased this apparatus.

There was no means in it whatever of keeping the pendulum of the same length when the heat should vary; consequently it was impossible that any accurate admeasurement of the different lengths of two pendulums keeping different times could be ascertained. Mr. WHITEHURST, indeed, had endeavoured to keep his pendulum of the same degree of heat; but I know from many experiments, among which some were for hatching eggs, how extremely difficult it is to maintain the same heat in any considerable mass, and that the means which may be employed to keep it within four or five degrees are almost totally inapplicable to pendulums; so that his experiments must have been defective. I therefore endeavoured to contrive a means of rendering the pendulum in his machine always of the same length, whatever the heat might be, by some addition to it. I thought of the principle, and formed the apparatus above described for this purpose.

It would be improper for me to repeat what has already been laid before this learned Society; therefore I shall only mention briefly, that the frame of Mr. WHITEHURST's machine was formed of two pieces of very clean well seasoned deal, to which was fixed the apparatus for rendering the wire flexible of which his pendulum was formed at the point Q, fig. 4, but there were no semicylindric pieces; the two square pieces came together, so as to make the top of the pendulum at their under surface; these pieces could be brought away from one another by the screw Y, so as to leave the wire free. The use of this was, by the screw L, fig. 4, to adjust the pendulum to its proper length, which has in this apparatus a considerable advantage, as it is not necessary, in the form I have given to this apparatus, to stop the clock in order to adjust it. These pieces of wood are mortised into a transverse piece of deal at the top and at the bottom firmly. Before I attempted to make a very perfect machine on these principles, I resolved to try how far this frame of wood might serve to connect the points I and E, fig. 1, and procured the apparatus for altering the point I, screwed on to one of these perpendicular pieces of wood on one side, and to the other on the other side. The pendulum itself serves as a plummet to place them perpendicular. In Mr. WHITEHURST's machine the screw at L, fig. 4, went through a piece of brass, and rested upon it, fixed to the top of the clock-case. But in my construction of it, when the length of the rod IB, fig. 1, is adjusted, the clock has nothing to do with the clock-case, excepting with that part of the wooden frame which connects the point I with the point E, fig 1. If I had been, or were to construct a machine for this purpose *ab origine*, instead of these two pieces of fir, I should employ a solid piece of brass, and make two cylindric cavities into it, parallel to one another, and in these cavities place two glass tubes, about two inches diameter, perpendicularly upwards, which may be done by various means; and, while in this situation, having heated them gradually to the heat of melted lead, I should pour in melted lead, so as to fix them in their places when it cooled. The apparatus for fixing the point I, and that for fixing the tube I I, fig. 2 and 3 at F, fig. 1, being also of brass, in heat they would always expand, and in cold contract, equally; so that the glass tubes would keep always at an equal distance from one another, and equally perpendicular. Glass is not only very little apt to contract and expand by heat, but free from any such disposition from moisture or dryness, which is not the case with wood.

Having added the apparatus I have described to Mr. WHITEHURST's machine, I set it a going, expecting, in the situation I placed it, only some approach towards accuracy in the length of the pendulum. I fixed beside it a transit which belonged to Mr. LUDLAM, the principal parts of which were made by Mr. RAMSDEN, the object-glass was a four-feet focus achromatic by DOLLOND. I found my meridian mark at about three quarters of a mile distance. I likewise borrowed, from my friend Mr. STEVENS, a clock with a gridiron pendulum, made by GRAHAM for his father Dr. STEVENS, in order to compare them together when I had no observations. There were several trivial circumstances, which baffled the experiments for some time, not worth relating, one only excepted; which was, that the curvature of the wire, acquired by its being wound round a pirn, was not entirely unfolded for some months, so that the clock went slower and slower during that time. At length this difficulty was overcome; I then began to observe with

GRAHAM's clock, in order to adjust the length of the pendulum, but found irregularities frequently take place. I then adjusted it by observation, and soon found that GRAHAM's clock went much more irregularly than my own. I adjusted it by turning the head of the screw at L, fig. 4, until the clock came to lose seven-tenths of a second in 24 hours. I did not think it worth while to bring it nearer; I then began to observe, and carried on the observations, when the weather permitted, for about nine months, during which the thermometer had fallen so low as 15° of FAHRENHEIT, in the clock case, and risen as high as 84; and with considerable variations. Unfortunately I have mislaid or lost the particulars of each observation; but I have preserved the greatest difference from the rate of its going. Counting on, according to the rate of its going, during the whole time it never exceeded the sum, half a second, nor was ever less than half a second, whether it was taken from day to day, month to month, or from any one to any other period during the observation.

Undoubtedly, therefore, notwithstanding the errors that might have arisen from the expansion of the wood by moisture, and from the unsteadiness of the building in which it was placed, it certainly performed better than any other time-piece that has been made; and perhaps affords a principle which may be used in fixed observatories for keeping time with certainty, by easy and not very expensive means; and of determining, with the rest of Mr. WHITEHURST's apparatus, the difference between the lengths of two pendulums swinging equal arches of circles of different diameters, in any two given different times.

The astronomer royal has also suggested an improvement:

viz. instead of grinding the two crystalline pieces in a cylindric form, the lower part should be ground in a cycloidal form; then it would have the advantage of cycloidal cheeks, which no contrivance hitherto has been able to attain.

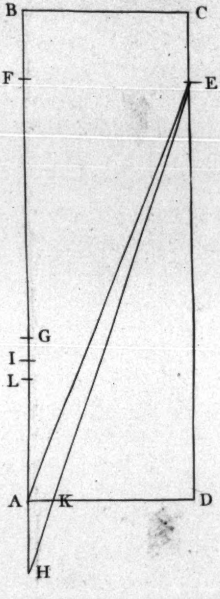
There are some farther observations necessary to be made, to enable workmen to construct clocks according to this principle, and some reflections upon its operation.

The manner of hanging a leaden weight to the pendulum, its proportion to the maintaining power, the manner of applying the pendulum to the clock, and the structure of the clock, are to be found in Mr. WHITEHURST's pamphlet; with only this difference, that the steel wire should go through a tube placed in the axis of the spherical lead weight, and be fixed at the bottom instead of the top of it. This, however, is of no great consequence if there be a power of altering the height of the fixed point I, fig. 1; because Mr. WHITEHURST's pendulum consisting partly of steel, partly of lead, therefore the point I must be adjusted to the joint expansions of lead and steel, if the wire be fixed at the top of the ball.

The first reflection that I shall make is, that the steel wire, the brass tube, and the materials which connect the points I E, being of different sizes, and different in their disposition to be heated or cooled, some one of them might be heated or cooled faster than another. But where good clocks are kept, the changes of the heat of the atmosphere are so slow, that no great difference can take place in the time that each of the parts rises to the heat of the atmosphere in the room where the clock is kept; none that could make any sensible error. As the difference of the time when they acquired the heat, would be compensated by the difference of the time when they acquired the cold, it could hardly happen that any sensible difference in the going of the clock could arise in any period of twenty-four hours, whether transits of the sun or of any of the fixed stars were taken.

The wire in each vibration hangs, during a certain portion of that vibration, between the two cylinders, and touches neither of them: during that time, the point Q must be considered as the top of the pendulum, not the slit between the cylinders; but this part of the vibration may be so very small a proportion of it, as not to make any sensible error; and it is accompanied on the other hand by a very great advantage. Except in Mr. ARNOLD's compensation for heat in watches, in all the other modes a surface or surfaces necessarily slide over one another; whenever this happens, if heat, by expanding one of the bodies, is to make its surface slide over the other, it has two things to accomplish, to overcome the *vis insita* of the matter, and the attraction of the two surfaces to one another. When then there is heat enough applied just to overcome the *vis insita*, it would not be sufficient to overcome the attraction likewise, excepting the matter was infinitely hard and inelastic. Although the heat therefore be increased, the compensating parts at first do not move so much as to overcome both these resistances, afterwards the parts jerk on suddenly, and in many cases go beyond what they otherwise would have done. As none of the expanding parts are to slide upon one another in Mr. ARNOLD's compensation, and there is a time in every vibration, in the apparatus above described, when none of the expanding parts slide over any thing, this disadvantage is avoided.

Fig. 1.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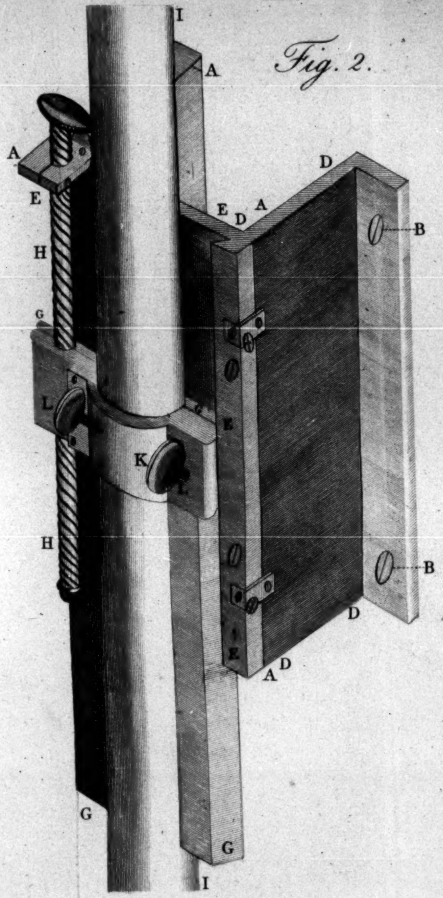
Philos. Trans. MDCCXCIV. *Tab.* II. *p.* 20.

Fig. 2.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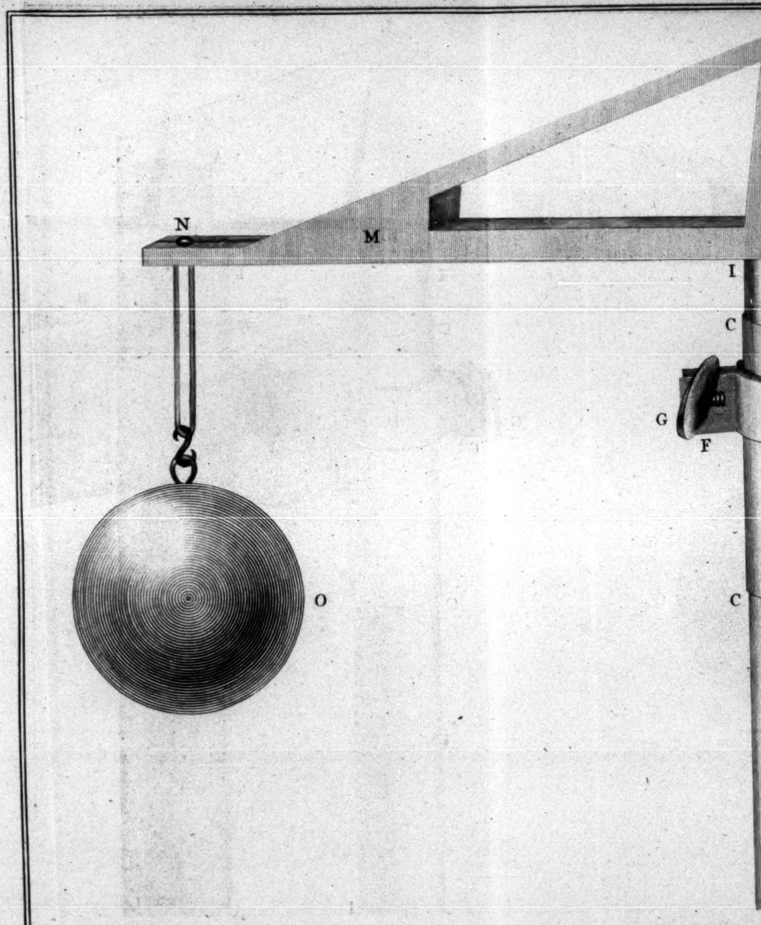
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Philos. Trans. MDCCXCIV. Tab. III. p. 20

Fig. 3.

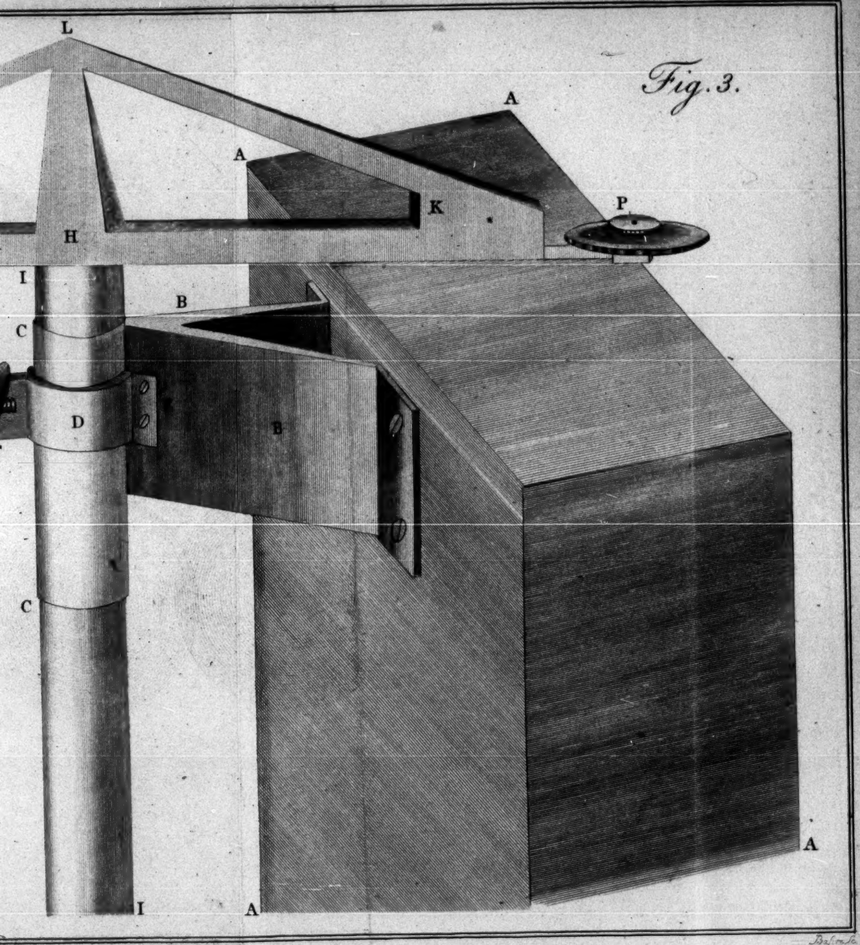
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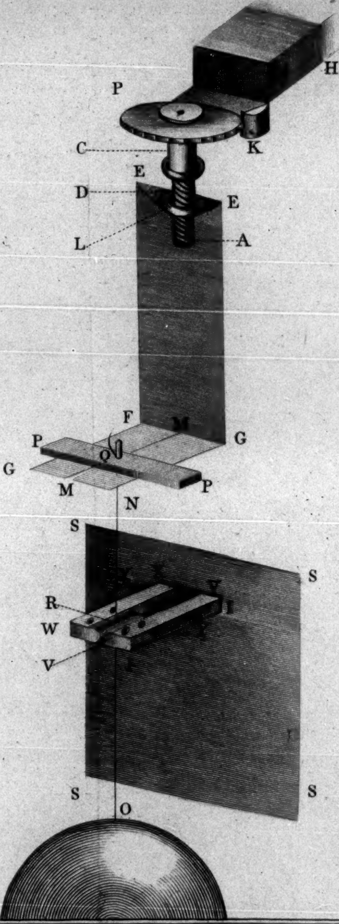
Fig. 4.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III. *Some Facts relative to the late Mr.* John Hunter's *preparation for the* Croonian *Lecture. By* Everard Home, *Esq. F. R. S.*

Read November 14, 1793.

Mr. Hunter having announced to the Royal Society that he would make the structure of the crystalline humour of the eye the subject of the Croonian lecture for the present year, and having, unfortunately for science, died before his observations on that subject were rendered complete, I feel it a duty I owe to his memory, as well as to the Society, to state the facts respecting this humour with which he had acquainted me; and shall subjoin an unfinished letter from Mr. Hunter to Sir Joseph Banks on the same subject.

It is now many years that Mr. Hunter has had an idea, that the crystalline humour was enabled by its own internal actions to adjust itself, so as to adapt the eye to different distances; and when the *Tænia hydatigena* first came under his observation as a living animal, he was surprised to see the quantity of contraction that took place in a membrane devoid of muscular fibres, but made use of the fact in his investigation of the structure of the crystalline humour of the eye.

Some time after this, having occasion to dissect the eye of the cuttle-fish, which he had frequently done before, but not with exactly the same view, he discovered in the crystalline humour a structure which corresponded with the idea he had formed of its actions in the human eye. He found it composed of laminæ, whose appearance was evidently fibrous, for some depth from the external surface; but becoming less and less distinct, till at last this fibrous appearance was entirely lost, and the middle, or central part of the humour, was compact and transparent, without any visible laminæ. From this structure it would appear, that in the eye of the cuttle-fish the exterior parts of the humour are fibrous, the interior parts not; so that the central part is a nucleus round which the fibrous coverings are placed. The preparations which demonstrate these facts will be laid before the Society.

As the structure of the crystalline humour in the cuttle-fish differs in nothing from that of the same humour in other animals, but in the distinctness of the fibrous appearance, Mr. HUNTER was led to consider that the exterior part in all of them was similar, although no appearance of fibres could be demonstrated.

What I have here explained, I was acquainted with at the time I had the honour of giving the CROONIAN lecture, in which I examined the different structures endowed with muscular action; and was desirous that Mr. HUNTER would, either of himself, or through me, communicate these observations to the Society; but this he declined doing till he had ascertained, by experiment, whether any muscular effect was really produced; and the hope of being assisted by Mr. RAMSDEN made him, from time to time, put off making his experiments.

In the course of this season he began his experiments, which were founded upon the analogy that ought to exist between this humour, if muscular, and others of a similar structure, which led him to expect that they would be acted upon by the same stimuli: and having found that a certain degree of heat, applied through the medium of water, will excite muscular action, after almost every other stimulus had failed, it was proposed to apply this to the crystalline humour, and ascertain its effects.

The crystalline humour taken from animals recently killed, must be considered as being still alive. Such humours were to be immersed in water of different temperatures, and placed in such a manner as to form the image of a lucid well defined object, by a proper apparatus for that purpose, so that any change of the place of that image from the stimulating effects of the warm water upon the humour would be readily ascertained. These were the experiments which Mr. HUNTER had instituted and begun; but in which he had not made sufficient progress to enable him to draw any conclusions.

To Sir JOSEPH BANKS, *from Mr.* HUNTER.

Sir,

When I did myself the honour of giving in my claim to the discovery of the crystalline humour being muscular, and proposed to make it the subject of the CROONIAN lecture, I did not foresee that any thing could prevent me from fulfilling my promise; but since that time, what with my state of health, which does not allow me to be very active; the hurry of official business on account of the war, and my brother-in-law, Mr. HOME, being employed on the medical staff, I have not had the power of repeating my experiments, and drawing out, to my satisfaction, the many conclusions which are the result of such a power in this humour.

The laws of optics are so well understood, and the knowledge of the eye, when considered as an optical instrument, has been rendered so perfect, that I do not consider myself capable of making any addition to it; but still there is a power in the eye by which it can adapt itself to different distances far too extensive for the simple mechanism of the parts to effect. This power writers upon this subject have been at great pains to investigate and explain. The motion of the crystalline humour forwards and backwards, was asserted by some to be the cause; while others supposed in the eye a power to alter its shape, so as to shorten or lengthen its axis, which altered the distance between the crystalline humour and the point of impression; but we should consider that a part of the eye is itself a refractor, and that if its shape be altered so as to remove the crystalline humour from the point of impression, in order to enable it to bring a distant object to its proper focus on the retina, this effect will be in some degree counteracted by the anterior part of the eye refracting more than before, by being rendered more convex. But we have, in fact, no power capable of producing this effect; for the straight muscles, so far from appearing to have this power, have been even supposed to flatten the eye, and shorten its axis: and it is very possible that the action of these muscles is such as tends to both effects; but being in opposition to each other, the eye retains its shape, the insertion of these muscles being much more forwards than appears to be necessary for the simple motions of the eye. Further, when we consider that in many animals the shape of the eye is unalterable, as in all of the whale tribe, the sclerotic coat being above half an inch thick, and composed of a strong tendinous substance. In many fish this coat is composed of cartilage; and in all birds the anterior part of it is (I believe) composed of bone. From all these considerations, I saw no power that could adapt the eye to the various distances of which we find it capable in the human body, unless we suppose the crystalline humour to be varied in figure, which can only be effected by a muscular action within itself. With this idea strongly impressed upon my mind, and finding that in many animals, when the crystalline humour was coagulated, it had a fibrous structure like muscles, I confess it seemed to me to confirm it; but as this might to others appear only conjecture, requiring some proof, I set about such experiments as were best adapted for that purpose. Knowing that in all violent deaths the muscles contract, I supposed the crystalline humour, if muscular, would show signs of this effect; for which purpose I got the eyes of bullocks when removed from the sockets, the moment the animal was knocked down, and while the eyes were warm the humours were removed."

Mr. HUNTER had proceeded thus far in the account of his experiments, when he was suddenly, and very unexpectedly, carried off; and as he has left no notes upon this subject, I am unable to make any addition to the account I have already given.

Mr. HUNTER's laying claim to the discovery of a fibrous structure in the crystalline humour, which had been observed long before, and described by the accurate LEUWENHOEK, may appear to require some explanation. The discovery of a fibrous appearance in that humour, appertains to LEUWENHOEK; but the discovery of an eye in which this structure of the crystalline humour was perfectly distinct, and in which all the circumstances, of course, and situation, could be determined, is due to Mr. HUNTER: and if it should be found by future observation and experiments, that this structure, which is different from any that has hitherto been described, is capable of producing consequent actions and effects, sufficient to explain the adjustment of the eye to different distances, it will not be considered as a small, or unimportant discovery.

The melancholy event which has deprived this learned Society of so valuable a member, and which has taken from me so able an instructor, so rare an example, and so inestimable a friend, is too recent, to make any apology necessary for the shortness, or incorrectness of this account. I thought it due to the memory of my friend, that no promise of his, however inadequate I feel myself to the performance, should be left unfulfilled; and the circumstances of distress, under which it has been drawn up, will procure for me every indulgence from this learned Society.

Leicester Square,
Nov. 4, 1793.

EVERARD HOME.

EXPLANATION OF THE PLATE, Tab. V.

Fig. 1st. A transverse section of the crystalline humour of the eye of a cuttle-fish, to shew its structure; the central part is transparent, but the others are opaque, having been coagulated by proof spirits; and give the appearance of distinct fibres surrounding the central part.

Fig. 1.

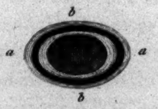
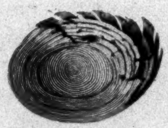


Fig. 2.



Basire Sc.

These fibres are not uniform circles or ovals, since the layers are of different thicknesses in particular parts; *aa* the fibres where they are most numerous; *bb* where they are least so.

Fig. 2d. A section of the crystalline humour, the central part being removed, to shew the fibrous structure of the surrounding laminæ.

IV. *Observations of a quintuple Belt on the Planet Saturn.* By William Herschel, *LL.D. F.R.S.*

Read December 19, 1793.

Every analogy that can be traced in the appearance of the planets, seems to throw some additional light on what we know of them already. In some of my former papers I have established the spheroidical form of the planet Saturn, and pointed out the motion of a spot on its disk. From the first of these may be inferred a considerable rotation on its axis; while the latter goes a step farther, and shews that it has such a motion. My late observations seem to hint to us, that the period in which it revolves is, probably, not of a long duration.

They are as follows:

Nov. 11, 1793. 3^h 35′, 7-feet reflector, power 287.

Close to the ring of Saturn, where it passes across the body of the planet, is the shadow of the ring; very narrow, and black. See Tab. VI. fig. 1.

Immediately south of the shadow is a bright, uniform, and broad belt.

Close to this bright belt is a broad, darker belt; which is divided by two narrow, white streaks; so that by this means, it becomes to be five belts; namely, three dark, and two bright ones; the colour of the dark belt is yellowish.

The space from the quintuple belt towards the south pole of the planet which is in view, is of a pale, whitish colour; less bright than the white equatorial belt, and much less so than the ring.

The globular form of Saturn is very visible, so that it has by no means the appearance of a flat disk.

Nov. 13, 3^h 30′. The quintuple belt on Saturn is as it was Nov. 11. I saw it three hours ago, and several times since, without any visible change.

Nov. 19, 3^h 14′. The southern belt of Saturn is still divided into five. The evening is not clear enough to observe changes in it, if there were any.

Nov. 22, 2^h 32′. The quintuple belt on Saturn remains still the same; power 287.

With 430, I see the same very distinctly, but the small divisions have hardly light enough when so much magnified.

I viewed the same belt with four different object specula. One of them shewed the divisions uncommonly well.

Dec. 3, 0^h 35′. 7-feet reflector; power 287. The quintuple belt upon Saturn remains as it was Nov. 22.

I tried several double and plano-concave eye-glasses, but found them all defective in figure except one, and that being of one inch focal length, the power was too low to expect seeing these belts well with it.

The smallness of the field of view, with astronomical objects is not so disagreeable as it is generally supposed to be; for the eye may have a motion before the lens, and by that means a small luminous object, when all the rest of the field is dark, and while the telescope remains in the same situation, may be seen for as long a time, passing through the field of a concave eye-glass, as it can in a convex one; whereas with the latter, it is well known that such a motion of the eye can be of no use.

2^h 36′. 20-feet reflector; power 157, 300, 480. I see the quintuple belt very well.

We know that the planet Jupiter has many belts. Some remarkable instances of their being very numerous are recorded in my journal, one of which is accompanied with a figure. The observations are as follows:

May 28, 1780. Jupiter's belts are curved; and there are a multitude of them all over the body of the planet. See fig. 2.

Jan. 18, 1790. I viewed Jupiter with the 40-feet reflector. There were two very dark, broad belts, divided by an equatorial zone or space, the colour of which was of a yellow cast. Next to the dark belts, on each side, towards the poles, were bright and dark small belts, alternately placed, and continued almost up to the poles, both ways.

In taking out fig. 2. from my journal, I perceive one so very unlike it just before, that I am induced to give it here, though rather foreign to my present purpose. It contains, however, an observation which it will not be amiss to record.

April 6, 1780. I had a fine view of Jupiter, and saw, as soon as I looked into the telescope, without having any previous notice of it, the shadow of the 3d satellite, and the satellite itself, upon the lower part of the disk. See fig. 3. The shadow was so black and well defined, that I attempted to measure it, and found its diameter by the micrometer 1″,562.

This measure of the shadow should be checked by the following observation.

March 15, 1792. 11^h 54′. With the 20-feet reflector, and a power of 800, I estimate the apparent diameter of the largest of Jupiter's satellites to be less than one-fourth of the diameter of the GEORGIAN planet, which I have just been viewing. With 1200, it seems also to be less, in the same proportion. With 2400, I can plainly perceive the disk of the satellite. With 4800, the apparent diameter of the largest of the satellites is less than one-quarter of that of the GEORGIAN planet.

The analogy alluded to in the first paragraph of this paper, refers to the numerous parallel belts which we have noticed, in the above given observations, on the disks of Jupiter and Saturn.

That belts are immediately connected with the rotation of the planets will hardly be denied, when those of Jupiter are so well known always to lie in the direction of its equatorial motion. Since, then, it appears that the belts of Saturn are very numerous, like those of Jupiter, and are also placed in the direction of the longest diameter of the planet, it may not be without some reason that we infer the period of the rotation of the former to be short, like that of the latter.

The planet Mars, in all my observations, never presented itself with any parallel belts, nor do we observe such phænomena on the disk of Venus. The first is known to have a rotation much slower than Jupiter;[*] and the latter, according to the accounts of CASSINI and BIANCHINI, is certainly not one that moves quickly upon its axis.

However, I do not mean to enter into the strength of an argument for a quick rotation of Saturn, that may be drawn from the condition of its belts. The circumstance of a quintuple belt, is adduced here with no other view, than merely to point out an analogy in the condition of the two largest planets of our system; and from thence to infer, that every conclusion on the atmosphere and rotation of the one, drawn from the appearance of its belts, will equally apply to the other.

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[*] See Phil. Trans. Vol. LXXI. Part I. page 134.

Slough, near Windsor,
Dec. 14, 1793.

WM. HERSCHEL.

Philos. Trans. MDCCXCIV. Tab. VI. p. 32.

Fig. 1.

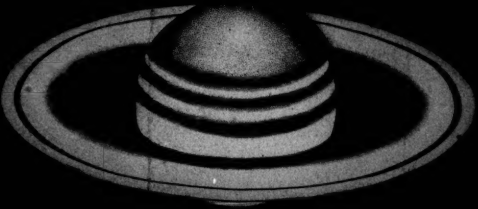


Fig. 2.

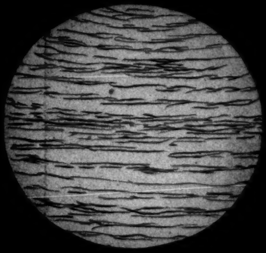
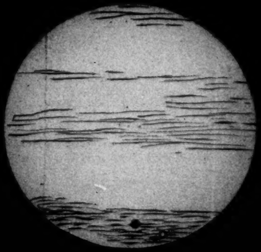


Fig. 3.



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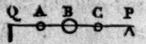
V. *Observations on the fundamental Property of the Lever; with a Proof of the Principle assumed by* Archimedes, *in his Demonstration. By the Rev. S. Vince, A. M. F. R. S.*

Read December 19, 1793.

THE want of a demonstration of the property of the lever, upon clear and self-evident principles, has justly been considered as a great desideratum in the science of mechanics, as the most important parts of that branch of natural philosophy are founded upon it. ARCHIMEDES was, I believe, the first who attempted it. He supposes, that if two equal bodies be placed upon a lever, their effect to turn it about any point is the same as if they were placed in the middle point between them. This proposition is by no means self-evident, and therefore the investigation which is founded upon it has been rejected as imperfect. HUYGENS observes, that some mathematicians, not satisfied with the principle here taken for granted, have, by altering the form of the demonstration, endeavoured to render its defects less sensible, but without success. He then attempts a demonstration of his own, in which he takes for granted, that if the same weight be removed to a greater distance from the fulcrum, the effect to turn about the lever will be greater; this is a principle by no means to be admitted, when we are supposed to be totally ignorant of the effects of weights upon a lever at different distances from the fulcrum. Moreover, if it were self-evident, his demonstration only holds when the lengths of the arms are commensurable. Sir I. NEWTON has given a demonstration, in which it is supposed, that if a given weight act in any direction, and any radii be drawn from the fulcrum to the line of direction, the effect to turn the lever will be the same on whichever of the radii it acts. But some of the most eminent mathematicians since his time have objected to this principle, as being far from self-evident, and in consequence thereof have attempted to demonstrate the proposition upon more clear and satisfactory principles. The demonstration by MAC LAURIN, as far as it goes, is certainly very satisfactory; but as he collects the truth of the proposition only from induction, and has not extended it to the case where the arms are incommensurable, his demonstration is imperfect. The demonstration given by Dr. HAMILTON, in his Essays, depends upon this proposition, that when a body is at rest, and acted upon by three forces, they will be as the three sides of a triangle parallel to the directions of the forces. Now this is true, when the three forces act at any point of a body; whereas, considering the lever as the body, the three forces act at different points, and therefore the principle, as applied by the author, is certainly not applicable. If in this demonstration we suppose a plane body, in which the three forces act, instead of simply a lever, then the three forces being actually directed to the same point of the body, the body would be at rest. But in reasoning from this to the case of the lever, the same difficulties would arise, as in the proof of Sir I. NEWTON. But admitting that all other objections could be removed, the demonstration fails when any two of the forces are parallel. Another demonstration is founded upon this principle, that if two non-elastic bodies meet with equal quantities of motion, they will after impact, continue at rest; and hence it is concluded, that if a lever which is in equilibrio be put in motion, the motions of the two bodies must be equal; and therefore the pressures of these bodies upon the lever at rest, to put it in motion, must be as their motions. Now in the first place, this is comparing the effects of pressure and motion, the relation of the measures of which, or whether they admit of any relation, we are totally unacquainted with. Moreover, they act under very different circumstances; for in the former case, the bodies acted immediately on each other, and in the latter, they act by means of a lever, the properties of which we are supposed to be ignorant of. When forces act on a body, considered as a point, or directly against the same point of any body, we only estimate the effect of these forces to move the body out of its place, and no rotatory motion is either generated, or any causes to produce it, considered in the investigation. When we, therefore, apply the same proposition to investigate the effect of forces to generate a rotatory motion, we manifestly apply it to a case which is not contained in it, nor to which there is a single principle in the proposition applicable. The demonstration given by Mr. Landen, in his Memoirs, is founded upon self-evident principles, nor do I see any objections to his reasoning upon them. But as his investigation consists of several cases, and is besides very long and tedious, something more simple is still much to be wished for, proper to be introduced in an elementary treatise of mechanics, so as not to perplex the young student either by the length of the demonstration, or want of evidence in its principles. What I here propose to offer will, I hope, render the whole business not only very simple, but also perfectly satisfactory.

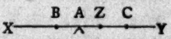
The demonstration given by Archimedes would be very satisfactory and elegant, provided the principle on which it is founded could be clearly proved; viz. *that two equal powers at the extremities, or their sum at the middle of a lever, would have equal effects to move it about any point.* Now, that the effects will be the same, so far as respects any *progressive* motion being communicated to the lever when at liberty to move freely, is sufficiently clear; but there is no evidence whatever that the effects will be the same to give the lever a *rotatory* motion about any point, because a very different motion is then produced, and we are supposed to know nothing about the efficacy of a force at different distances from the fulcrum to produce such a motion. Besides, the two motions are not only different, but the *same* forces are known to produce *different* effects in the two cases; for in the former case the two *equal* powers at the extremities of the arms produce *equal* effects in generating a *progressive* motion; but in the latter case they do *not* produce *equal* effects in generating a *rotatory* motion. We cannot therefore reason from one to the other. The principle, however, may be thus proved.

Let A C, be two equal bodies placed on a straight lever, A P moveable about P; bisect A C in B, produce P A to Q, and take B Q = B P, and suppose the end Q to be sustained by a prop. Then as A and C are similarly situated in respect to each end of the lever, that is, A P = C Q, and A Q = C P, the prop and fulcrum must bear equal parts of the whole weight; and therefore the prop at Q will be pressed with a



weight equal to A. Now take away the weights A and C, and put a weight at B equal to their sum; and then the weight at B being equally distant from Q and P, the prop and fulcrum must sustain equal parts of the whole weight, and therefore the prop will now also sustain a weight equal to A. Hence if the prop Q be taken away, the moving force to turn the lever about P in both cases must evidently be the same; therefore the effects of A and C upon the lever to turn it about any point are the same as when they are both placed in the middle point between them. And the same is manifestly true if A and C be placed without the fulcrum and prop. If therefore A C be a cylindrical lever of uniform density, its effect to turn itself about any point will be the same as if the whole were collected into the middle point B; which follows from what has been already proved, by conceiving the whole cylinder to be divided into an infinite number of laminæ perpendicular to its axis, of equal thicknesses.

The principle therefore assumed by ARCHIMEDES is thus established upon the most self-evident principle, that is, that *equal* bodies at *equal* distances must produce *equal* effects; which is manifest from this consideration, that when *all* the circumstances in the cause are equal, the effects must be equal. Thus the whole demonstration of ARCHIMEDES is rendered perfectly complete, and at the same time it is very short and simple. The other part of the demonstration we shall here insert, for the use of those who may not be acquainted with it.



Let X Y be a cylinder, which bisect in A, on which point it would manifestly rest. Take any point Z, and bisect Z X in B, and Z Y in C; then, from what has been proved, the effects of the two parts ZX, ZY to turn the lever about A is the same as if the weight of each part were collected into B and C respectively, which weights are manifestly as ZX, ZY, and which therefore conceive to be placed at B and C. Now $AB = AX - XB = \frac{1}{2}XY - \frac{1}{2}XZ = \frac{1}{2}YZ$; and $AC = AY - YC = \frac{1}{2}XY - \frac{1}{2}ZY = \frac{1}{2}XZ$; consequently $AB : AC :: \frac{1}{2}YZ : \frac{1}{2}XZ :: YZ : XZ ::$ the weight at C : the weight at B.

The property of the straight lever being thus established, every thing relative to the bent lever immediately follows.

VI. *Account of some Particulars observed during the late Eclipse of the Sun. By* William Herschel, *LL.D. F. R. S.*

Read January 9, 1794.

It will be proper to remark that my attention, in observing this eclipse, was not directed to the time of the several particulars which are usually noticed in phænomena of this kind; such as the beginning, the end, and the digits eclipsed. I was very well assured that the care of other astronomers would render my endeavours, in that respect, perfectly unnecessary. The only view I had was, to avail myself of the power and distinctness of my telescopes, in order to see whether any appearances would arise that might deserve to be recorded; and the following particulars will, at least, serve to point out the way for similar observations to be made in other eclipses, where different circumstances may chance to afford an opportunity for gathering some addition to our knowledge, with regard to the nature and condition of the moon, or of the sun, and perhaps of both these heavenly bodies.

Sept. 5, 1793. $8^h\,40'\,3''$ by the clock.* My attention being directed to the place where I supposed the first impression would be made, I perceived two mountains of the moon enter the disk of the sun, as delineated at *a*, *b*, fig. 1, Tab. VII. The time of their beginning to appear, when I saw them first, might be one or two seconds past.

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* By account, my sidereal time-piece was about $5'\,1'',7$ too forward; but, as no transits had been lately taken, there may be an error of some seconds.

$9^h\ 5'$. 7-feet reflector; power 287. The internal, luminous angle made on the sun, by the intersection of the limb of the moon, which is now but little more than a rectangle, is perfectly sharp up to the very point. It is not in the least disfigured by the refraction of the lunar atmosphere. The present shape of the angle, however, is not favourable for shewing the effects of that atmosphere.

$9^h\ 17'$. The luminous angles of the sun's preceding and following limbs, which are now acute, remain perfectly sharp. One of them, indeed, was disfigured, a little while ago, by the entrance of a mountain of the moon, but is now restored to its sharpness.

$10^h\ 5'$. I delineated the appearance of the limb of the moon upon the sun, and found its mountains as in fig. 2. At *a* was a large *table mountain*, as it may be called, from its flat appearance; at *b* and *c* were elevated, pointed rocks. Their appearance changing pretty fast, no great accuracy can be expected in their expressed relative situation.

I suppose the height of the most elevated of these mountains not to exceed a mile and an half; for, on drawing several of them upon the segment of a large circle, so as to look like what they appeared when projected upon the sun, I found them to be from the 1500dth to the 2000dth part of the diameter of that circle. Then, putting the moon's diameter, as M. DE LA LANDE states it, at 782 French leagues, or 2151 English miles, we find the 1500dth part of this to be less than one mile and an half for the highest; and the 2000dth part, not quite one mile and a tenth for the lowest.

I attended all this time to the appearance of the sharp limb *a b c* of the sun, fig. 3, and suspected, sometimes, a little bending of the cusps outwards, as expressed at *b* in fig. 4; but upon long, and attentive inspection, I could not satisfy myself of its reality. If there was a bending, it did probably not amount to one second of a degree; for, having formerly been much in the habit of measuring the moon's mountains,* the quantity of one second, on its disk, was still familiar enough to me to estimate it pretty exactly.

10ʰ 15′. I looked out with the natural eye for the planet Venus, and soon perceived her. In the telescope, with 287, she appeared very sharp and well defined, and was a little gibbous.

It may seem, perhaps, extraordinary that in the trial above mentioned, the eye should be able to ascertain the proportion of a quantity so little as the fifteen hundredth, or two thousandth part of the diameter of the moon; but the experiment may be easily repeated in the following manner:

Upon a line, six or eight inches long, drawn on a sheet of paper, make several small marks, representing mountains on the projected circumference of a large globe. The paper being then placed in a proper light and situation, withdraw the eye to the distance of 7, 8, or 9 feet, and take notice which of the marks appear of the same size, and distinctness, with the mountains they represent. Then, from the known angular magnitude of the moon, calculate its diameter, at the distance of your situation; this, multiplied by the power of the telescope, gives the diameter of a circle, to the circumference of which belongs the line, upon which are placed the marks above described. Now, measure the elevation of these marks, above that line, and you will obtain the proportion they bear to the diameter of the circle.

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* In the years 1779, 1780, and 1781. I did not measure, I suppose, less than an hundred mountains of the moon, in which I used three different methods: the projection of the tops of these mountains beyond the enlightened part of the disk; the length of their shadow on the surface of the moon; and their perpendicular projection on the full edge of the moon's limb. Some of these observations are contained in a former paper (see Phil. Trans. Vol. LXX. Part II. page 507); but most of them remain uncalculated in my journal, till some proper opportunity.

In my experiment, I found that I could plainly see some small protuberances at 9 feet distance, which were no higher than the 50th part of an inch. Then putting the diameter of the moon at 30′, we have the sum of the logarithms of the tangent of 30′; of the power 287; and of the 50ths of an inch contained in 9 feet; which, taken from the logarithm of the diameter of the moon in miles, gives the logarithm of ,16. By which we find, that so small a mountain as the $\frac{16}{100}$dth, or not much more than the sixth part of a mile, may be perceived and estimated, by the telescope and power that was used upon this occasion; and that, consequently, the estimation of mountains, near a mile and an half high, must become a very easy task.

Slough, near Windsor,
Dec. 30, 1793.

WM. HERSCHEL.

Philos. Trans. MDCCXCIV. Tab. VII. p. 42.

Fig. 1.

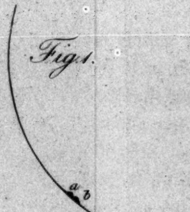


Fig. 2.

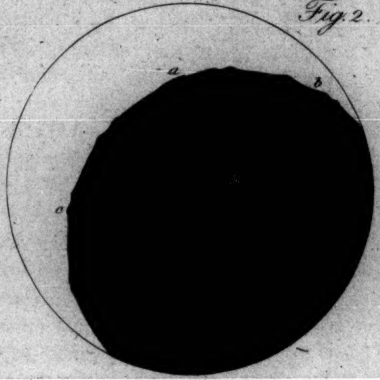


Fig. 3.

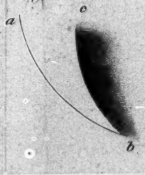
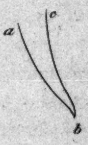


Fig. 4.



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VII. *The Latitudes and Longitudes of several Places in* Denmark; *calculated from the Trigonometrical Operations. By* Thomas Bugge, *F. R. S. Regius Professor of Astronomy at* Copenhagen.

Read January 9, 1794.

THE geographical surveying of Denmark was begun in the year 1762. The foundations of geographical maps are the trigonometrical operations, or great triangles, whose bases were measured with deal rods. The angles of the triangles were observed with a circular instrument of 1 foot radius; the divisions of this instrument are double, in 90 and 96 degrees. With this instrument the angles may be observed to a less error than 8″, and the sum of all the angles in every triangle very seldom have had a difference of 15″ from 180 degrees. For this reason the bases, measured at several places in Seland and Jutland, have very well agreed with the corresponding sides, computed through a long series of triangles, begun from the observatory at Copenhagen. I believe that a distance, found by those trigonometrical operations, is to be depended upon to $\frac{1}{20000}$th part of the whole. I beg leave to observe, that the Danish astronomers and geographers, for 31 years, have been before hand in making use of circular instruments, which now begin to be of a more general use in astronomical and geographical observations. The royal observatory at Copenhagen has, since the year 1781, been adorned with a circular instrument of 4 feet radius, which, at least at that time, was the only circular instrument of that size.

By the trigonometrical operations, the meridian of Copenhagen, and of several other places, and a perpendicular to the meridian of the observatory, are drawn. The special position of villages, farms, and cottages, the situation of the coast, woods, rivers, ponds, moors, roads, are laid down by the plain table, on a scale of 2000 Danish or Rhenish feet to one decimal inch. After a reduction to $\frac{1}{8}$th part, to a scale of 1 Danish mile to 2 inches, we have published 9 geographical maps, which, as well for the geometrical exactness, as for the beauty of engraving, seem not to be unworthy of the approbation of foreigners.

I have described the instruments, and the methods of our geometrical surveying, and of the trigonometrical operations, in a treatise published in the Danish language at Copenhagen 1779, and translated into the German by Major Aster at Dresden, 1787. In this paper I only shall lay before the Royal Society a new method of computing the longitude and the latitude of places, laid down by trigonometrical operations.

Let EAIH (Tab. VIII.) be an ellipsis; EH half the less axis; IH half the greater axis; A the observatory at Copenhagen; AV its vertical line; the angle V the complement to the latitude of the observatory. Then by the nature of the ellipsis. $AV = \frac{HI^2}{(HI^2 \text{ sin.}^2 V + HE^2 \text{ cos.}^2 V)^{\frac{1}{2}}}$. AN is a great circle, perpendicular to the meridian of Copenhagen. The tangent to the same meridian $AF = AV \times \text{tang. } V$. A *m n o p* . . . D is a series of triangles in the direction of the parallel of Copenhagen. *g t u r x* . . . G is a series of triangles in the direction of the meridian GDE of a place G, whose longitude and latitude are to be calculated. For the first series of triangles may be taken the parallel, divided into small parts $AB = BC = CD$; and for the second series may be taken the meridian GD; because the arches of those circles are known by the triangles, and computed from the trigonometrical operations. FAGD in the 1st. fig. is laid down in fig. 2 upon a plane. The angles a, b, c, d, are equal to the angles A, B, C, D. The lines af, bf, cf, gdf, are equal to AF, BF, CF, GDF, touching the meridians AE, BE, CE, GDE in A, B, C, D. The angle $dfa = DFA = AFB + BFC + CFD$. For the place g, or G, are given the distance from the meridian of Copenhagen $= gk$, and the distance from the perpendicular $= ak$, and $af = AF = AV \times \text{tang. } V$. In the case that g is more southerly than an, then $fk = af + ak$. If the place is northerly, then $fk = af - ak$. Hence tang. $dfa = \frac{gk}{fk}$. The complement to the angle dfa is the angle $fna = fgk$, which the meridian gdf makes with the perpendicular to the meridian of Copenhagen. Now $DEA : DFA = AF : AM = \text{tang. } V : \text{sin. } V$; therefore *the longitude of the place* G *from the meridian of Copenhagen* $= DEA = DFA \times \frac{\text{tang. } V}{\text{sin. } V} = \frac{DFA}{\text{cos. } V}$.

Again, $gf = \frac{gk}{\text{sin. } afd} = \frac{fk}{\text{sin. } g}$. If the place g is more southerly than the perpendicular an, then $dg = gf - fd = gf - af$; if more northerly than an, in that case $dg = af - fg$. From hence *the latitude of the place g may be found.*

The following Table contains the latitudes of towns and places, with their longitudes from the royal observatory at Copenhagen, calculated from our trigonometrical operations.

Names of Places.	Sea or Country.	Latitude.	Longitude.	
			In Degrees.	In Time.
		° ′ ″	° ′ ″	h ′ ″
Landscrone	Sweden	55 52 23	0 15 16	0 1 0,5 E.
Hveen (church)	— —	55 54 38	0 5 56	0 0 23,75 E.
Kullen (light-house)	— —	56 18 3	0 7 58	0 0 31,75 W.
Frankeklint (cape)	Langeland	55 9 44	1 38 47	0 6 35 W.
Kongsberg (cape)	Möen	54 58 3	0 4 12	0 0 16,75 W.
Sproe (isle)	Belt	55 19 56	1 37 0	0 6 35,25 W.
Copenhagen (observatory)	Seland	55 41 4	0 0 0	0 0 0
Roeskilde (cathedral)	— —	55 38 25	0 29 48	0 1 59,75 W.
Holbek (church)	— —	55 43 2	2 51 26	0 3 25,75 W.
Kallundborg (church)	— —	55 40 54	1 29 12	0 5 56,75 W.
Korsör (light-house)	— —	55 20 22	1 27 0	0 5 48 W.
Nestved (church)	— —	55 13 55	0 49 12	0 3 16,75 W.
Wordingborg (tower)	— —	55 0 32	0 40 4	0 2 40,25 W.
Ringsted (church)	— —	55 26 51	0 47 20	0 3 9,25 W.
Skagen (light-house)	North Jutland	57 43 44	1 57 55	0 7 51,7 W.
Hiöring (church)	— —	57 27 44	2 35 17	0 10 21,1 W.
Fladstrand (church)	— —	57 27 3	2 2 15	0 8 9,0 W.
Sæbye (church)	— —	57 20 2	2 2 36	0 8 10,5 W.
Aalborg (St. Budolph)	— —	57 2 57	2 39 4	0 10 36,3 W.
Nibe (church)	— —	56 59 4	2 55 54	0 11 43,6 W.
Grenaae (church)	— —	56 24 57	1 41 49	0 6 47,3 W.
Randers (highest steeple)	— —	56 27 48	2 32 3	0 10 8,2 W.
Viborg (cathedral)	— —	57 27 11	3 9 25	0 12 37,7 W.
Aarhuus (cathedral)	— —	56 9 35	2 21 40	0 9 26,6 W.
Ribe (cathedral)	— —	55 19 57	3 48 25	0 15 13,7 W.
Hadersleben (church)	South Jutland or Schleswig	55 15 15	3 4 56	0 12 19,7 W.
Norborg (highest steeple)		55 3 53	2 49 53	0 11 19,6 W.
Apenrade (St. Nicolas)	— —	55 2 57	3 9 7	0 12 36,9 W.
Tondern (Christ.)	— —	54 56 30	3 41 53	0 14 47,5 W.
Sönderborg (St. Mary)	— —	54 54 59	2 47 1	0 11 8,1 W.
Flensborg (highest steeple)	— —	54 47 18	3 8 5	0 12 32,3 W.
Husum (church)	— —	54 28 29	3 31 3	0 14 4,2 W.
Gluckstad (highest steeple)	Holstein	53 47 44	3 8 43	0 12 34,8 W.
Hesselöe (isle)	Kattegat	56 11 46	0 51 44	0 3 27 W.
Anholt (light-house)	— —	56 44 20	0 55 24	0 3 41,5 W.

In all the best maps of the Kattegat, as that by Mr. LOUS, published at Copenhagen, 1790, that by M. VERDUN DE LA CRENNE, M. BORDA, and M. PINGRE', Paris, 1778, that by Mr. AKELEIE, Copenhagen, 1771, that by Mr. ANKERKRONA,

Philos. Trans. MDCCXCIV. *Tab.* VIII. *p.* 46.

Fig. 1.

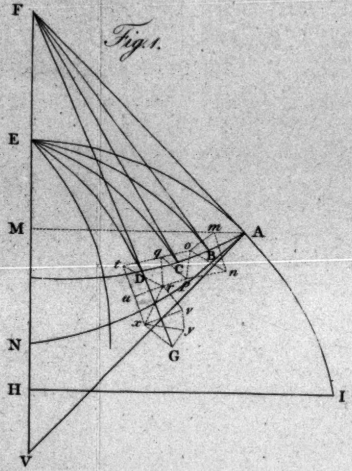
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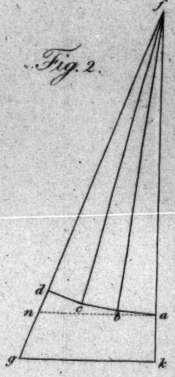
Fig. 2.



Stokholm, 1782, the position of Anholt is very erroneous. *The light-house of Anholt, and the whole isle, is from 7 to 9 minutes too much westerly; and the distance from the light-house to the Swedish coast, in a direction perpendicular to the meridian of the light-house is, in all maps hitherto published, nearly 4 English miles, or $\frac{1}{8}$th part of the whole too great.* Experience has taught the navigators, that they come too soon down upon Anholt; or that they, cruising between Anholt and Sweden, overrun their reckoning, which was ascribed to the currents; although the true reason of it was the great error in the geographical and hydrographical position of Anholt in a narrow and dangerous passage.

VIII. *On the Rotation of the Planet Saturn upon its Axis.* By William Herschel, *LL. D. F. R. S.*

Read January 23, 1794.

In a late paper on the multiplicity of the regular belts of the planet Saturn, I pointed out an analogy, which might lead us to surmise that it had a pretty quick rotation upon its axis; I can at present announce the reality of that rotation. The following series of observations, in which Saturn has been traced through one hundred and fifty-four revolutions of its equator, will sufficiently confirm it.

The changes in the belts of Jupiter, it is well known, are so frequent, that we find some difficulty to make our observations of them agree to within 3, 4, or 5 minutes of time; but the belts on Saturn, which I have been lately observing, seem to have undergone no very material change, during the course of the two last months; so that we may hope the period of the rotation of this planet, which will be assigned in this paper, may be looked upon as having a considerable degree of exactness.

Before we can enter into particulars, it will be necessary to give the series of observations upon which my computations have been founded. It is not sufficient to extract only those parts of them which have served for calculating the period; as the value of astronomical observations consists in having them entire; every circumstance, as it occurred, is of consequence, and facts being stubborn things, we cannot decide upon them properly till they have been entirely laid open to our view, and sufficiently scrutinized. For this purpose the observations are all extracted from the journal, in the regular order in which they were made; and here I must remark, that I purposely avoided any calculations, or even surmises, of the length of a rotation, while the observations were making; in order to be perfectly free from every bias that might mislead the eye. In this I succeeded so well, that, when I began to calculate, I mistook not less than 4 hours and $\frac{1}{4}$ in the first supposition I made; which, happening to agree extraordinarily well with four of the most pointed observations, it misled me so far, that I was very near rejecting the whole series as inconsistent, and began to think the changes in the belts to have been so frequent, and irregular, as not to fall under any kind of calculation. It will, however, soon appear that this has not been the case, and that, on the contrary, there has been more steadiness and regularity in the belts, than might well have been expected in such kind of appearances.

OBSERVATIONS ON THE BELTS OF SATURN.*

Nov. 11, 1793. 3^h 35′. *(Correction of the clock* — 7′ 27″,1).† Seven-feet reflector; power 287; new specula, uncommonly distinct.* Close to the ring of Saturn, where it passes the body of the planet, is the shadow of the ring; very narrow, and black.

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* A few of these observations have been lately given; but as they are essential to the series, I thought it better to repeat them here, than to refer to my former paper.

† My time is kept by one of Shelton's clocks, set now and then by equal altitudes, taken with a 12-inch Bird's quadrant; and checked by the passage of a set of stars over the wire of a four-feet telescope, firmly fixed to the wall of my house. By calling the correction minus, I denote, in this case, that the clock is 7′ 27″,1 too fast for true sidereal time.

Immediately south of the shadow is a bright, uniform, and broad belt.

Close to this belt is a broad, darker belt, which is divided by two narrow, white streaks; so that, by this means, it becomes to be five belts; namely, three dark, and two bright ones; the colour of the dark belt is yellowish.† (A)

The space from the quintuple belt towards the south pole of the planet which is in view, is of a pale whitish colour; less bright than the white equatorial belt, and much less so than the ring.

The globular form of Saturn is very visible, so that it has, by no means, the appearance of a flat disk.

Nov. 13. $3^h\ 30'$ (*Cor.* — $7'\ 29'',5$.) The quintuple belt on Saturn is as it was Nov. 11. I saw it three hours ago, and several times since, without any visible change. (B)

Nov. 19. $3^h\ 14'$. (*Cor.* — $7'\ 36'',8$.) The southern belt of Saturn is still divided into five; the evening is not clear enough to observe changes in it, if there were any. (C)

Nov. 22. $2^h\ 32'$ (*Cor.* — $7'\ 40'',4$.) The quintuple belt on Saturn remains still the same; power 287. (D)

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* In the course of these observations, I made 10 new object specula, and 14 small plain ones, for my 7-feet reflector; having already found, that with this instrument I had light sufficient to see the belts of Saturn completely well; and that, here, the maximum of distinctness might be much easier obtained, than where large apertures are concerned.

† The letters (A) (B) (C), (*a*) (*b*) (*c*), &c. as they occur, refer to calculations which will be given hereafter.

With 430, I see the same very distinctly; but the small divisions have hardly light enough, when so much magnified.

I viewed the same belt with four different object specula. One of them shewed the divisions uncommonly well.

Dec. 3. 0h 35′. *(Cor. — 7′ 53″,8.)* The quintuple belt upon Saturn remains as it was Nov. 22. (E)

2h 36′. 20-feet reflector; power 157; 300; 480. I see the quintuple belt very well.*

Dec. 4. 23h 22′. *(Cor. — 7′ 55″ by a transit.)* 10-feet reflector. The quintuple belt is on Saturn, as it was last night. (F)

4h 57′. 7-feet. The quintuple belt is uncommonly distinct.† The three narrow dark belts are of an equal breadth over the whole disk; the two bright belts which divide them, are also of an equal breadth throughout, but a little narrower than the dark ones. (G)

5h 58′. I see the quintuple belt so clearly defined throughout all the 3 dark, and 2 bright belts, that I am apt to guess that the side which presents itself to me now is not the same which I saw in the first part of the evening; but it is not easily possible to determine whether the air might not be less clear then. I saw all other phænomena on Saturn extremely well, many times, between 1ʰ and 3ʰ; but not the belts so well as now. (H)

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* I found that the strong light of this instrument was too great a fatigue for the eye, which cannot bear to look at a very luminous object for a long time together. For this reason, I chiefly used the 7-feet reflector; and in future, all the observations, not expressly marked otherwise, are to be understood as having been made with that instrument; bearing an eye-glass of 3-tenths of an inch focal length. My object specula are generally from 84 to 88 inches in focus, and, therefore, give a power from 280 to 293. The favourite one gave 287. I had another reason for chiefly confining myself to one instrument, and one power; which was, that every circumstance being as much as possible the same, a change in the object I viewed might be the sooner perceived.

† When I found the divisions between the small belts so remarkably distinct, I began to suspect that they might not be, all around the planet, perfectly uniform in brightness and figure; and therefore now described the phænomena that occurred more minutely and carefully.

6ʰ 36′. The belt remains as free from interruptions as it was at 5ʰ 58′. (I)

6ʰ 52′. I see the planet not so well defined now, as I did in the first part of the evening; being at present nearer the horizon; but I see the belts better than I did at that time. (K)

Dec. 6. 22ʰ 28′ (*Cor.* — 7′ 57″,2.) I see the quintuple belt very distinctly. (L)

22ʰ 55′. I see the quintuple belt as readily as I see the rest of the appearances on Saturn. (M) I took care to bend my head so, as to receive the picture of the belt in the same direction upon the retina, as I did December 4, at 5ʰ 58′.*

23ʰ 55′. I now see the quintuple belt full as well as I did Dec. 4, at 5ʰ 58′. (N)

1ʰ 25′. That part of the quintuple belt which is now on the meridian, or centre of Saturn, is much less separated and defined than what was there at 23ʰ 55′. (O)

2ʰ 26′. The divisions in the quintuple belt are not grown distincter, but rather more confused than they were before; I can scarcely perceive them. (P)

3ʰ 28′. The uppermost of the small dark belts, in that part which is on the meridian, is very faint, and the most north is rather darker than before. (Q)

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* This was a precaution that occurred to me, as there was a possibility that the vertical diameter of the retina might be more or less sensible than the horizontal one; but I had no reason afterwards to suppose that any such difference really exists.

4ʰ 28′. The most north of the two belts is darker, and a little broader than the most south. (R)

5ʰ 53′. Saturn is remarkably distinct; much more so than Dec. 4, at 5ʰ 58′; but the quintuple belt is less distinct than it was that evening; it has also undoubtedly a different appearance. The northern belt is the darkest and broadest, that next to it is less dark; and the third, or southmost belt, is faint, and hardly to be seen; the narrow white belts that separate them are contracted, and but just visible. (S)

OBSERVATION UPON THE DOUBLE RING OF SATURN.*

The outer ring is less bright than the inner ring. The inner ring is very bright close to the dividing space; and, at about half its breadth, it begins to change colour, gradually growing fainter; and just upon the inner edge, it is almost of the colour of the dark part of the quintuple belt.

7ʰ 52′. This is evidently another part of the planet than what I saw in the beginning of the evening. (T)

Dec. 9. 5ʰ 33′. *(Cor. — 8′ 0″,4.)* The quintuple belt is extremely distinct, but not so much so, in proportion to the rest of the appearances, as I might expect. (U)

6ʰ 9′. The southmost dark belt is very faint; the northmost is the strongest and broadest; the bright divisions are very small, and difficult to be seen; I can, however, trace them all along. (V)

Dec. 11. 1ʰ 25′. *(Cor. — 8′ 2″,6.)* I see the quintuple belt. The southmost belt is extremely faint; that to the north is the darkest and broadest; the middlemost is nearly as dark, but not quite so broad. (W) The air is much disturbed by wind, and flying haziness.

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* This observation is foreign to the present purpose, but as it is new, and but short, I would not omit it; and for the same reason, two or three more are retained hereafter.

$5^h\ 5'$. The quintuple belt is very distinct. The southmost belt is less faint than it was at $1^h\ 25'$; but the wind is too high, and the air too disturbed, to examine it minutely. (X)

Dec. 13. $23^h\ 40'$. (*Cor.* — $8'\ 4'',7$.) I see the divisions of the quintuple belt very well; but there is a dry wind, and the telescope will not shew objects with that degree of distinctness which it usually does, when moisture is discharged from the air, by the precipitation of dew. (Y)

$0^h\ 46'$. I see the quintuple belt very well.

REMARK ON THE SHADOWS OF SATURN AND ITS RING.

On the south following part of the ring, close to the body of the planet, is the shadow of the body.

The shadow of the ring upon the body of the planet close to the ring, is not parallel to the ring at the two extremes, but a little broader there, than in the middle; the ends turning towards the south.

$2^h\ 4'$. The bright divisions between the belts are very narrow. The southmost dark belt is not much less faint than the northmost. (Z)

$2^h\ 51'$. The southmost dark belt on the preceding side, which at $23^h\ 40'$, I thought was a little more south than the inside of the ring, now falls short of it. The broad bright belt also seems to be narrower now, than it was at that time.*

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* Suspecting that the situation and direction of the belts might not be uniform all around the planet, I began to be more careful in describing the particulars relating to those circumstances; but found, soon after, that no additional light could be gathered from an attention to them.

3ʰ 2′. The broad bright belt, is as broad as the next three belts and a half, of the quintuple belt: that is, not quite so broad as the quintuple belt, without the southmost narrow dark belt.

4ʰ 11′. The broad white belt is of the breadth of the three adjoining belts.

5ʰ 11′. Appearances seem to be the same as they were an hour ago. The evening is indifferent. (*a*)

Dec. 16. 0ʰ 43′. *(Cor. — 8′ 8″,0 by a transit.)* I see the quintuple belt extremely well.

1ʰ 3′. The most south of the three dark belts, is full up to the inner edge of the preceding side of the ring; or rather a little above (or south of) it. The following side is less south, with regard to the ring, than the preceding; so that the quintuple belt is not parallel with that part of the ring, which crosses the body.

2ʰ 21′. The southmost belt is very faint; the middle and northern ones are much darker, and seem to be broader and closer, than I have at other times seen them. (*b*)

With 215. This power is too small to distinguish the divisions of the quintuple belt sufficiently.

3ʰ 12′. There seems to be no material alteration, but I do not see the divisions of the belt so distinctly as from the appearance of the other phænomena I ought to do. (*c*)

4ʰ 5′. The southmost belt meets the inner edge of the preceding part of the ring very nearly, and the quintuple belt seems to be parallel to the northern part of the ring, or nearly so.

4ʰ 16′. 10-feet reflector; power 300. The quintuple belt is parallel to the northern part of the ring, which is turned towards us.

5^{h} 35′. 7-feet. The southmost belt is brighter than it was at 2^{h} 21′. (*d*)

Dec. 18. 23^{h} 54′. (*Cor.* — 8′ 9″,8.) I see the quintuple belt. (*e*)

1^{h} 19′. All the parts of the quintuple belt seem to be very uniform. The southmost on the preceding side, reaches full up to the ring, where it passes behind the body. (*f*)

I viewed the planet with eight different object specula, they all shewed the quintuple belt very well.

Dec. 19. 1^{h} 41′. (*Cor.* — 8′ 10″,6.) I have a very beautiful view of Saturn. I should suppose this part of Saturn to be that, where the quintuple belt is not so distinct as it is on some other parts; though I see it very well; yet from the extraordinary distinctness of the other phænomena, I think I ought to see it still better. (*g*)

THE FIVE OLD SATELLITES.

1^{h} 56′. 10-*feet reflector*; *with a power of* 60 *only, I see all the five old satellites.*[*]

4^{h} 15′. 7-feet reflector. The southmost belt is faint The quintuple belt is a little broader on the preceding side, than on the following. (*b*)

Dec. 23. 3^{h} 46′. (*Cor.* — 8′ 14″,2.) I see the quintuple belt very distinctly. The northmost of the dark belts is the broadest and darkest; the southmost is very faint; they are parallel to the ring, and the southmost just comes up to the inner edge of the ring. (*i*)

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[*] This observation was made to try with how low a power they might be seen.

Dec. 29. 1ʰ 10′. (*Cor.* — 8′ 19″,5.) The quintuple belt remains on Saturn as it used to be; the southmost of the dark belts is faint. (*k*)

Jan. 1, 1794. 2ʰ 2′. (*Cor.* — 8′ 22″,1.) I see the quintuple belt very well; the southmost belt is not much fainter than the northmost. (*l*)

OBSERVATIONS OF THE SOUTH POLE OF SATURN, AND THE SHADOW OF THE RING.

3ʰ 40′. *The south polar regions of Saturn, are a little brighter in proportion to the bright equatorial belt, than they used to be; they are almost as bright as that belt.*

The shadow of the ring upon Saturn is perfectly black, like the shadow of Saturn upon the ring. The shadow of the ring upon Saturn, on each side, is bent a little southwards; so that the apparent curve it makes departs a little from the ring.

5ʰ 18′. The three dark belts of the quintuple belt seem to be very close, but the air is tremulous; I can however see them divided. (*m*)

Jan. 2. 23ʰ 53′. (*Cor.* — 8′ 23″,0 *by a transit.*) The quintuple belt is very distinct. The southmost belt is almost as dark as the other two. (*n*)

1ʰ 52′. The southmost belt is very nearly, if not quite as dark and distinct as the northmost. The air is very fine, and all the phænomena on Saturn are beautifully distinct. (*o*)

Jan. 4. 1ʰ 36′. (*Cor.* — 8′ 23″,8.) The quintuple belt is not so distinct as, from the appearance of the rest of the phænomena on Saturn, one might expect to see it. All the three dark belts are fainter than I have often seen them; and the southmost of them is much fainter than the northmost. (*p*)

2^h^ 35′. The quintuple belt is still faint. The southmost belt much fainter than the northmost; the latter is not only stronger, but also broader than the former. (*q*)

Jan. 7. 1^h^ 4′. (*Cor.* — *8′ 25″,0 by a transit.*) The three dark belts of the quintuple belt, seem not to differ much in colour and breadth, but the evening is very indifferent. (*r*)

1^h^ 53′. The air is a little clearer. The southmost is very little (if at all) less dark than the northmost; they are all very faint. (*s*)

3^h^ 18′. As well as the night will permit to see, I judge the dark belts to be pretty equal in colour and size, the northmost, however, is still a little darker, and broader than the other two. (*t*)

3^h^ 44′. The dark belts seem to be as equal as I have seen them at any time. I see them very well. (*u*)

TRIAL OF CONCAVE EYE GLASSES.

I tried five new concave eye glasses, but they all proved defective in figure; with one of them, power 360, I saw the quintuple belt pretty well. With regard to the field of view they are full as convenient as convex glasses. (*v*)

5^h^ 50′. The three dark belts are still nearly alike, and uniformly divided by the bright ones. (*w*)

Jan. 16. 2^h^ 52′. (*Cor.* — 8′ 28″,6.) I suppose this to be the part of the quintuple belt, which is nearly uniform; the southmost one however is not quite so dark as the northmost. (*x*)

4^h^ 20′. The belts seem to be equal and uniform throughout. (*y*)

5^h^ 5′. The belts seem to be uniform; the southmost however is the faintest. (*z*)

DETERMINATION OF THE PERIOD.

I shall now enter upon the method, which has been used to determine the rotation of the planet, from these observations.

Let K, T, Z, P, in the annexed figure, (see Tab. IX.) represent the quintuple belt on the southern hemisphere of Saturn; the different parts of which are diversified as expressed by the different tints of the belts: those at E A K N I H M L G being uniform, while others at T B C D V, and R Q Y F have the southmost of the small dark belts very faint, and the northmost pretty strongly marked. Let α be the south pole of Saturn, and let the circle 90, 180, 270, 360 represent its equator, divided into degrees; so that α 180, α 210, α 240, α 270, &c. are meridians of Saturn, which, as the planet turns upon its axis, from west to east, will successively pass over the line $\alpha\ \beta\ \gamma$, representing that meridian on Saturn which passes through the earth.

Then the eye of the observer being placed in the line $\alpha\ \beta\ \gamma$, at a great distance, and the hemisphere of Saturn, which is here projected on the plane of the equator, being in an oblique position, will only see the semicircle $\delta\ \beta\ \epsilon$. But on account of the great inclination of the arches $\zeta\ \delta$, $\eta\ \epsilon$, to the visual ray $\gamma\ \beta\ \alpha$, the eye will not perceive minute divisions, or marks, till they come within the limits $\zeta\ \eta$, and even then will no where judge so well of their brightness and figure as when they draw near the situation β.

The divisions on the equator, are to serve us to point out those places of the quintuple belt, which, by future calculations of the motion of this equator, will be shewn to have been on the meridian, $\alpha\ \beta\ \gamma$, at any given time; and the numbers are placed in the reverse order of the rotation, that the calculated motion of the belt may immediately point out the place which is come to the meridian. Thus, if the point K at 180 has moved 53 degrees forward, the situation *x*, on the belt, may be concluded to be on the meridian; because it is at 180 + 53, that is, the 233d degree.

Assuming for an epoch the observation of Dec. 4. 4^h 57′, where the different small belts, that make up the quintuple belt, are described as quite uniform; let it be placed at the 180th degree of the divided circle, where it will be fully exposed to the view of the observer.

I now select a few observations that are strongly marked, and as far distant from each other as can be found, by way of trying in what rotation they will agree. Such are the following two: Dec. 4. 6^h 52′; Jan. 7. 3^h 44′, for the places where the belts were uniform; and other two, Dec. 6. 7^h 52′, Jan. 4. 2^h 35′, for places where they appeared unequal. The sidereal times being corrected, and brought to true mean time, we have from Dec. 4. 13^h 46′ 51″, to Jan. 7. 8^h 25′ 11″ an interval of 33 days 18^h 38′ 20″; in which time let us suppose 79 revolutions to have been made. This will give 10^h 15′ 40″ for the time of each revolution.

In the next place, we have from Dec. 6. 14^h 38′47″ to Jan. 4. 7^h 28′ 12″ an interval of 28 days 16^h 49′ 25″; and allotting 67 revolutions to this, we obtain 10^h 16′ 51″ for the time of the rotation. These periods being independent of each other, and the observations having been made upon different parts of the belt, agree very well together. But now, some intermediate places are wanted, by way of trying, whether the period thus determined will accord with the rest of the observations; and for this purpose I select Dec. 18. $1^h\,19'$, Jan. 2. $1^h\,52'$; and bringing them also to true mean time, we have,

From Dec. 4. $13^h\,46'\,51''$ to Dec. 18. $7^h\,19'\,28''$ an interval of 13 days $17^h\,32'\,37''$. Then, supposing 32 revolutions to have been made, we obtain a period of $10^h\,17'\,54''$. Also,

From Dec. 4. $13^h\,46'\,51''$ to Jan. 2. $6^h\,53'\,11''$ is an interval of 28 days $17^h\,6'\,20''$; and admitting 67 revolutions of the belts, the period will be $10^h\,17'\,6''$.

These trials of intermediate times agreeing with the former sufficiently well, there can remain no doubt about the true quantity of the period in general. I therefore take a mean of the two first determinations, which gives $10^h\,16'\,15'',5$ for the approximate rotation of Saturn upon its axis.

It now becomes necessary to construct tables for a general calculation of all the observations. For, if these should contain descriptions contradicting the calculated appearances of the quintuple belt, our assigned period could not be looked upon as sufficiently established; on the contrary, if the calculated and observed appearances are found to agree, we may rest satisfied that the rotatory motion of this planet, which has so long eluded our strictest attention, is at length obtained.

In consequence of a few trials, which were made after the 7th of January, by tables constructed upon this mean period, I found that some small correction was required; and obtaining another very good observation on the 16th of the same month, it gave me an interval which included one hundred revolutions of the equator of Saturn. Now, making the proper deduction for the planet's retrograde motion during the time that passed between the first and last observation, we have from Dec. 4. $13^h\,46'\,51''$ to Jan. 16. $8^h\,25'\,39''$ an interval of 42 days

18^h 38′ 48″ in which the equator of Saturn moved over 35998,87 degrees, from which we compute a period of 10^h 16′ 0″,44.

The following tables have been constructed upon this last period, and in the use of them the complement of the geocentric longitude of Saturn is always to be added, as has been explained in the tables of the satellites of that planet. Phil. Trans. Vol. LXXX. part II. page 495.

TABLES FOR THE EQUATORIAL MOTION OF SATURN.

Epochs of the Longitude of the uniform Part of the Belts.

1793 November	-	284°,18
1793 December	-	330 ,62
1794 January	-	138 ,61.

Motion of the Equator of Saturn, in Days, Hours, Minutes, and Seconds.

Days.	Deg. dec.	Hours	Deg. dec.	Min.	Deg.dec.	Min.	Deg.dec.	Sec.	dec.	Sec.	dec.
1	121,55	1	35,06	1	,58	31	18,12	1	,01	31	,30
2	243,10	2	70,13	2	1,17	32	18,70	2	,02	32	,31
3	4,64	3	105,19	3	1,75	33	19,29	3	,03	33	,32
4	126,19	4	140,26	4	2,34	34	19,87	4	,04	34	,33
5	247,74	5	175,32	5	2,92	35	20,45	5	,05	35	,34
6	9,29	6	210,39	6	3,51	36	21,04	6	,06	36	,35
7	130,84	7	245,45	7	4,09	37	21,62	7	,07	37	,36
8	252,38	8	280,52	8	4,68	38	22,21	8	,08	38	,37
9	13,93	9	315,58	9	5,26	39	22,79	9	,09	39	,38
10	135,48	10	350,65	10	5,84	40	23,38	10	,10	40	,39
11	257,03	11	25,71	11	6,43	41	23,96	11	,11	41	,40
12	18,58	12	60,77	12	7,01	42	24,54	12	,12	42	,41
13	140,12	13	95,84	13	7,60	43	25,13	13	,13	43	,42
14	261,67	14	130,90	14	8,18	44	25,71	14	,14	44	,43
15	23,22	15	165,97	15	8,77	45	26,30	15	,15	45	,44
16	144,77	16	201,03	16	9,35	46	26,88	16	,16	46	,45
17	266,32	17	236,10	17	9,93	47	27,47	17	,16	47	,46
18	27,86	18	271,16	18	10,52	48	28,05	18	,17	48	,47
19	149,41	19	306,23	19	11,10	49	28,64	19	,18	49	,48
20	270,96	20	341,29	20	11,69	50	29,22	20	,19	50	,49
21	32,51	21	16,35	21	12,27	51	29,80	21	,20	51	,49
22	154,06	22	51,42	22	12,86	52	30,39	22	,21	52	,50
23	275,60	23	86,48	23	13,44	53	30,97	23	,22	53	,51
24	37,15	24	121,55	24	14,03	54	31,56	24	,23	54	,52
25	158,70			25	14,61	55	32,14	25	,24	55	,53
26	280,25			26	15,19	56	32,73	26	,25	56	,54
27	41,80			27	15,78	57	33,31	27	,26	57	,55
28	163,34			28	16,36	58	33,90	28	,27	58	,56
29	284,89			29	16,95	59	34,48	29	,28	59	,57
30	46,44			30	17,53	60	35,06	30	,29	60	,58
31	167,99										

The following 50 positions have been calculated by the above tables, and belong to the several observations which are marked with the tabular letters of references, as has been explained in the 2d note, page 50. They have been computed to two places of decimals, but are only put down in degrees, as being sufficiently near for comparing them with the descriptions belonging to those parts of the belt which they point out.

Table of calculated Positions.

A	201	K	180	T	94	*c*	43	*m*	225	*v*	186
B	77	L	125	U	10	*d*	127	*n*	155	*w*	239
C	63	M	141	V	31	*e*	167	*o*	224	*x*	129
D	37	N	176	W	105	*f*	216	*p*	93	*y*	180
E	201	O	228	X	233	*g*	348	*q*	128	*z*	206
F	278	P	264	Y	282	*h*	78	*r*	73		
G	113	Q	300	Z	6	*i*	178	*s*	101		
H	149	R	335	*a*	115	*k*	83	*t*	151		
I	171	S	24	*b*	14	*l*	111	*u*	166		

I may venture to say, that there are not many of these calculated observations which do not very forcibly concur in proving the assigned revolution to be properly stated. I shall however only mention a few of the leading ones, and leave the rest to be looked over at leisure by those who wish to examine the subject minutely.

In observations previous to the 4th of December, no particular attention had been given to the minutiæ of the belts; but we may suppose them, on the 11th of November, to have been considerably distinct and uniform, to occasion their being noticed; and this the calculation verifies. For the position A points out 201 degrees, as the situation which was on the meridian at the time of observation, and by the figure we find it to be a very marked place.

A strong evidence of the rotation is the position H 149, observed Dec. 4, contrasted with the place F 278, which had been viewed in the early part of the same evening. The calculation here completely supports the suspicion which is expressed in the observation, "that the side which presented "itself then was not the same which had been seen in the be- "ginning of the evening."

The observations of Dec. 6, are of the most decisive nature; as will clearly appear by viewing the calculated positions L M N O P Q R S T, and comparing them with the descriptions belonging to them, that have been given in the observations. For, here the revolving belts were successively seen, in all their various tints, and the last position T was marked down as leaving no doubt of the evident rotation. By the calculation it appears that the belts had moved over 329 degrees, in the course of this evening's observation.

When the positions *c*, *d*, are compared, which were observed Dec. 16, we see that the southmost belt had acquired an additional brightness, as the observation expresses. It may not be amiss to remark upon this occasion, that brightness relates to clearness, distinctness, and easiness of vision; in opposition to faintness and confused outlines; therefore, the belt being brighter here, denotes its being more strongly marked by a deeper tint of dusky yellow, and by clearer divisions; so as to be easier perceived.

Dec. 19 furnishes a good instance of the exactness of our period; as the calculated position *g* perfectly justifies the surmise which is expressed in the observation.

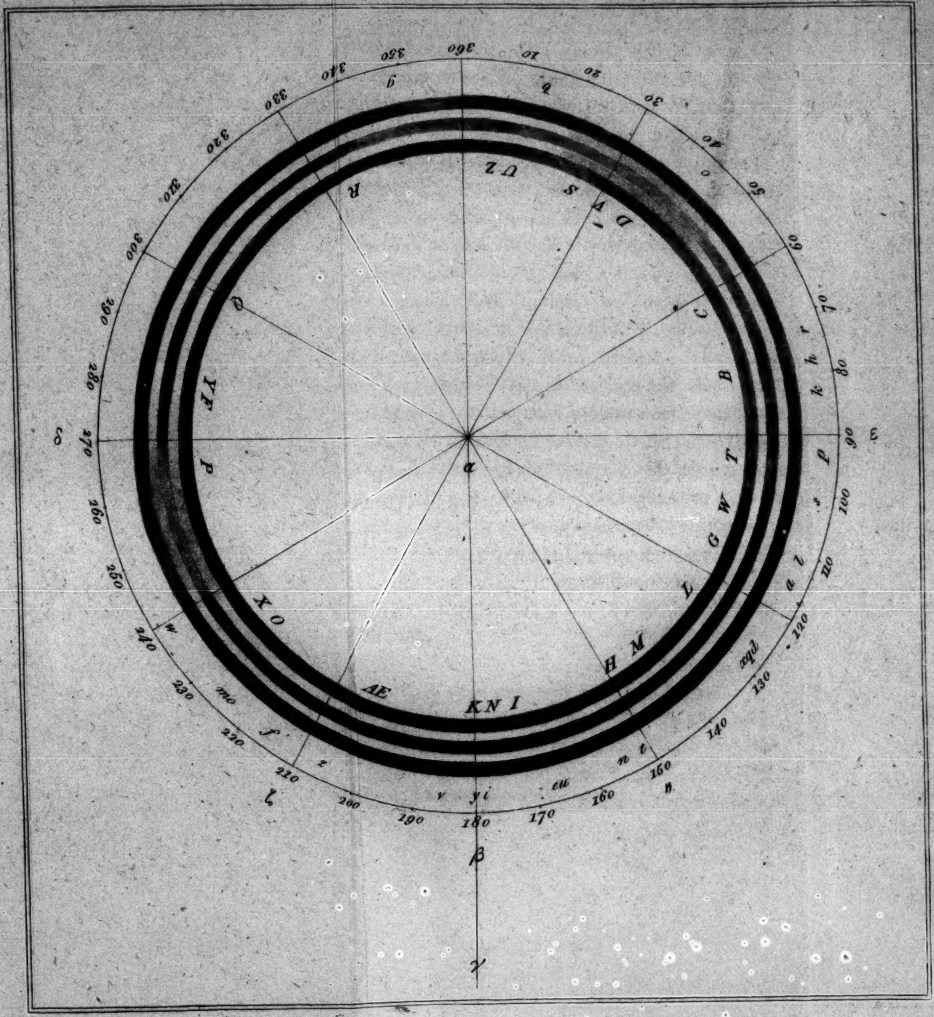
Last of all, the place *y*, being truly pointed out by computation for Jan. 16, after a series of an hundred revolutions since the 4th of December, must concur in supporting our assigned period.

I shall only add one general remark, which is, that if we lengthen the time of the rotation but 2 minutes, it will throw the last observation back above 116 degrees; and if we diminish it by 2 minutes, there will arise an excess of more than 117; and, in either case, the calculations and observations would be totally at variance: from which we may conclude that our period must be exact to much less than 2 minutes, either way. Indeed, what alterations may have taken place in the belts themselves, it is impossible to determine. That there have been some, we may admit, and rather suppose, but we have no particular reason to suspect them to have been very considerable. And, after we have shewn that a proper motion, in the spots of the belts, of 116 degrees one way, or of 117 the other, would only occasion an error of 2 minutes in time, we need not hesitate to fix the rotation of the planet Saturn upon its axis at $10^{h}\ 16'\ 0'',4$.

Slough, near Windsor,
Jan. 22, 1794.

WM. HERSCHEL.

Erratum. Phil. Trans. for 1793. Part II. page 215. 2d line, for *more than two degrees and a third*, read 1° 11′ 47″,6.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IX. *An Account of a Method of measuring the comparative Intensities of the Light emitted by luminous Bodies. By Lieutenant-General Sir* Benjamin Thompson, *Count of* Rumford, *F. R. S. In two Letters to Sir* Joseph Banks, *Bart. P. R. S.*

Read February 6, 1794.

LETTER I.

DEAR SIR,

ABOUT two years ago, being employed in making a number of experiments to determine, if possible, the most œconomical method of lighting up a very large workhouse, or public manufactory, which has been erected in the suburbs of this city under my directions, where the poor, old and young, and all industrious people who are in want of work, are employed in a great variety of different manufactures, a method occurred to me for measuring the relative quantities of light emitted by lamps of different constructions, candles, &c. which is very simple, and which I have reason to think perfectly accurate. I sent you a verbal account of this little invention about a year and a half ago, by Dr. BAADER, who wrote to me that you seemed to be pleased with it; but, as I think it very probable, that he might not have been able to give you a complete idea of the matter from memory, I have determined to send you a short account of it in writing, which, if you should think it worthy of that honour, you will be pleased to lay before the Royal Society. The method is shortly this:—

Let the two burning candles, lamps, or other lights to be compared, A and B, be placed at equal heights upon two light tables, or moveable stands, in a darkened room; let a sheet of clean white paper be equally spread out, and fastened upon the wainscot or side of the room, at the same height from the floor with the lights, and let the lights be placed over against this sheet of paper, at the distance of 6 or 8 feet from it, and 6 or 8 feet from each other, in such a manner, that a line drawn from the centre of the paper, perpendicular to its surface, shall bisect the angle formed by lines drawn from the lights to that centre; in which case, considering the sheet of paper as a plane speculum, the one light will be precisely in the line of reflection of the other.

This may be easily performed, by actually placing a piece of a looking-glass, 6 or 8 inches square, flat upon the paper, in the middle of it, and observing by means of it the real lines of reflection of the lights from that plane, removing it afterwards as soon as the lights are properly arranged.

When this is done, a small cylinder of wood, about $\frac{1}{4}$ of an inch in diameter, and 6 inches long, must be held in a vertical position about 2 or 3 inches before the centre of the sheet of paper, and in such a manner, that the two shadows of the cylinder corresponding to the two lights may be distinctly seen upon the paper.

If these shadows should be found to be of unequal densities, which will almost always be the case, then that light whose corresponding shadow is the densest, must be removed farther off, or the other must be brought nearer to the paper, till the densities of the shadows appear to be exactly equal; or in other words, till the densities of the rays from the two lights are equal *at the surface of the paper*; when, the distances of the lights from the centre of the paper being measured, the squares of those distances will be to each other as the real intensities of the lights in question at their sources.

If, for example, the weaker light being placed at the distance of 4 feet from the centre of the paper, it should be found necessary, in order that the shadows may be of the same density, to remove the stronger light to the distance of 8 feet from that centre, in that case, the real intensity of the stronger light will be to that of the weaker as 8^2 to 4^2; or as 64 to 16; or 4 to 1; and so for any other distances.

It is well known, that any quality proceeding from a centre in straight lines in all directions, like the light emitted by a luminous body, its intensity at any given distance from that centre will be as the square of that distance inversely; and hence it is clear, that the intensities of the lights in question at their sources, must be to each other as the squares of their distances from that given point *where their rays uniting are found to be of equal density.* For putting $x =$ the intensity of B; if P represents the point where the rays from A and from B meeting are found to be of equal density or strength, and if the distance of A from P be $= m$, and the distance of B from the same point P $= n$; then, as the intensity of the light of A at P is $= \frac{x}{m^2}$, and the intensity of the light of B at the same place $= \frac{y}{n^2}$, and as it is $\frac{x}{m^2} = \frac{y}{n^2}$ by the supposition, it will be $x : y :: m^2 : n^2$.

That the shadows being of equal density at any given point, the intensities of the illuminating rays must of necessity be equal at that point also, is evident from hence, that the total absence of light being perfect blackness, and the shadow corresponding to one of the lights in question being deeper or fainter, according as it is more or less enlightened by the other, when the shadows are equal, the intensities of the illuminating rays must be equal likewise.

In removing the lights, in order to bring the shadows to be of the same density, care must be taken to recede from, or advance towards the centre of the paper in a straight line, so that the one light may always be found exactly in the line of reflection of the other; otherwise the rays from the different lights falling upon the paper, and consequently upon the shadows, at different angles, will render the experiment fallacious.

When the intensity of one strong light is compared with the intensities of several smaller lights taken together, the smaller lights should be placed in a line perpendicular to a line drawn to the centre of the paper, and as near to each other as possible; and it is likewise necessary to place them at a greater distance from the paper than when only single lights are compared.

In all cases, it is absolutely necessary to take the greatest care that the lights compared be properly trimmed, and that they burn clear, and equally, otherwise the results of the experiments will be extremely irregular and inconclusive. It is astonishing what a difference there is in the quantities of light emitted by the same candle, when it burns with its greatest brilliancy, and when it has grown dim for want of snuffing. But as this diminution of light is progressive, and as the eye insensibly conforms to the quantity of light actually present, it is not always taken notice of by the spectators;—it is nevertheless very considerable, in fact, as will be apparent to any one who will take the trouble to make the experiment; and so great is the fluctuation in the quantity of light emitted by burning bodies, lamps, or candles, in all cases, even under the most favourable circumstances, that this is the source of the greatest difficulties I have met with in determining the relative intensities of lights by the method here proposed.

To ascertain by this method the comparative densities, or intensities of the light of the moon, and of that of a candle, the moon's direct rays must be received upon a plane white surface, at an angle of incidence of about 60°, and the candle placed in the line of the reflection of the moon's rays from this surface; when the shadows of the cylinder corresponding to the moon's light, and to that of the candle, being brought to be of equal density, by removing the candle farther off, or bringing it nearer to the centre of the white plane, as the occasion may require, the intensity of the moon's light will be equal to that of the candle *at the given distance of the candle from the plane.*

To ascertain the intensity of the light of the heavens by day or by night, this light must be let into a darkened room through a long tube, blackened on the inside, when its intensity may be compared with that of a candle or lamp by the method above described.

To determine the intensity of the direct rays of the sun, compared to the light emitted by any of our artificial illuminators, it may perhaps be necessary, considering the almost inconceivable intensity of the sun's light, to make use of some further contrivances and precautions, but I am convinced, however, that it may be done, and that even with a very considerable degree of precision. And when the relative intensity of the sun's light at the surface of the earth, compared with the intensity of the light of a given lamp, placed at a given distance, and burning with a flame of given dimensions, shall be known; it will then be easy, from the known size and distance of the sun, to compute the relative density of his light at *his* surface, compared to the density of the light of the flame of the lamp at the surface of that flame.

The intensity of the light emitted in the combustion of iron or of phosphorus in dephlogisticated air, as also that of all other burning, or red-hot bodies, may be compared and determined by this method with the greatest facility and exactness.

In my next letter I shall endeavour to give you an account of the result of my inquiries with respect to the best and most œconomical method of producing light by candles, lamps, &c. for common use; together with a comparative view of the expence of lights of various kinds, the *quantity* of light produced remaining the same; with such further remarks and observations as may occur.

I am, &c.

Munich,
20th December, 1792.

LETTER II.

DEAR SIR,

Since my letter of the 20th December last I have made many improvements in the apparatus contrived for measuring the intensity of light, and I have now brought the principal instrument to such a degree of perfection, that, if I might without being suspected of affectation, I should dignify it with a name, and call it a *photometer.* I have likewise made a considerable number of new experiments; but before I proceed to give an account of them, it will be necessary to describe very particularly the alterations I have found it expedient to make in the instruments.

And, in the first place, the shadows, instead of being thrown upon a paper spread out upon the wainscot, or side of the room, are now projected upon the inside of the back part of a wooden box, 7¼ inches wide, 10½ inches long, and 3¼ inches deep, in the clear, open in front to receive the light, and painted black on the inside, in every part except the back, upon which the white paper is fastened which receives the shadows. To the under part of the box is fitted a ball and socket, by which it is attached to a stand which supports it; and the top or lid of it is fitted with hinges, in order that the box may be laid quite open as often as it is necessary to alter any part of the machinery it contains. The front of the box is likewise furnished with a falling lid or door, moveable upon hinges, by which the box is closed in front when it is not in actual use.

Finding it very inconvenient to compare two shadows projected by the same cylinder, as these were either necessarily too far from each other to be compared with certainty, or when they were nearer they were in part hid from the eye by the cylinder, to remedy this inconvenience, I now make use of two cylinders; which being fixed perpendicularly in the bottom of the box just described, in a line parallel to the back part of it, distant from this back $2\frac{2}{10}$ inches, and from each other 3 inches, measuring from the centres of the cylinders; when the two lights made use of in the experiment are properly placed, these two cylinders project four shadows upon the white paper upon the inside of the back part of the box, which I shall henceforth call *the field* of the instrument, two of which shadows are in contact precisely in the middle of that field, and it is these two alone that are to be attended to. To prevent the attention being distracted by the presence of unnecessary objects, the two outside shadows are made to disappear, which is done by rendering the field of the instrument so narrow, that they fall without it upon a blackened surface, upon which they are not visible. If the cylinders be each $\frac{4}{10}$ of an inch in diameter, and $2\frac{2}{10}$ inches in height, as in the instrument I have lately constructed, it will be quite sufficient, if the field be $2\frac{7}{10}$ inches wide; and as an unnecessary height of the field is not only useless, but disadvantageous, as a large surface of white paper not covered by the shadows produces too strong a glare of light, the field ought not to be more than $\frac{3}{10}$ of an inch higher than the tops of the cylinders.

In order to be able to place the lights with facility and precision, a fine black line is drawn through the middle of the field from the top to the bottom of it, and another (horizontal) line at right angles to it, at the height of the top of the cylinders. When the tops of the shadows touch this last-mentioned line, the lights are at a proper height; and when further, the two shadows are in contact with each other in the middle of the field, the lights are then in their proper directions.

In my new-improved instrument (for I have already caused four to be constructed) the white paper which forms the field is not fastened immediately upon the inside of the back of the box, but it is pasted upon a small pane of very fine ground glass, and this glass, thus covered, is let down into a groove made to receive it in the back of the box. This covered glass is $5\frac{1}{2}$ inches long, and as wide as the box is deep, viz. $3\frac{1}{4}$ inches, but the field of the instrument is reduced to its proper size by a screen of black pasteboard interposed before the anterior surface of this covered glass, and resting immediately upon it. A hole in this pasteboard, in the form of an oblong square, $1\frac{7}{10}$ inches wide, and two inches high, determines the dimensions, and forms the boundaries of the field. This screen should be large enough to cover the whole inside of the back of the box, and it may be fixed in its place by means of grooves in the sides of the box, into which it may be made to enter. The position of the opening above mentioned is determined by the height of the cylinders, the top of it being $\frac{1}{10}$ of an inch higher than the tops of the cylinders; and as the height of it is only two inches, while the height of the cylinders is $2\frac{1}{10}$ inches, it is evident that the shadows of the lower parts of the cylinders do not enter the field. No inconvenience arises from that circumstance; on the contrary, several advantages are derived from that arrangement.

Instead of the screen just described, I sometimes make use of another, which differs from it only in this, that the hole in it, which determines the form and dimensions of the field, instead of being quadrangular, is round, and $1\frac{6}{10}$ inches in diameter. And when this screen is made use of, the shadows are increased in width (by means which will hereafter be described), in such a manner as completely to fill the field, appearing under the form of two hemispheres, or rather half disks, touching each other in a vertical line. The object I had in view in reducing the field and the shadows to a circular form was this; I imagined that by diminishing the number of objects capable of acting upon the mind, and particularly by removing all straight lines and angles, and all unnecessary varieties of lights and shades, the attention might be concentrated and fixed in such a manner as to render the sense of sight peculiarly acute in distinguishing any difference in the simple objects presented to the eye. But however plausible this reasoning may appear, I own the experiment did not answer my expectation. It is true, the apparent densities of two equal hemispheres of shade, in contact with each other, may be compared with great facility, and when no discernible difference is to be perceived between them, it is more than probable that they are in fact very nearly equal; but still I have found by experience, that two equal parallelograms of shade in contact with each other may be compared with the same ease, and, I have reason to think, with equal certainty, and *that* even when these united shadows are bounded on three sides by a perfectly white surface, illuminated by the direct rays of two strong lights; that is to say, when the screen with the quadrangular opening or field is made use of.

In describing the cylinders by which the shadows are projected, I said they were fixed in the bottom of the box; but as the diameters of the shadows of the cylinders vary in some small degree, in proportion as the lights are broader or narrower, and as they are brought nearer to or removed farther from the photometer, in order to be able in all cases to bring these shadows to be of the same diameter, which I have found by experience to be advantageous, in order to judge with greater facility and certainty when the shadows are of the same density, I have rendered the cylinders moveable about their axes, and have added to each a vertical wing $\frac{11}{20}$ of an inch wide, $\frac{1}{16}$ of an inch thick, and of equal height with the cylinder itself, and firmly fixed to it from the top to the bottom. This wing commonly lies in the middle of the shadow of the cylinder, and as long as it remains in that situation it has no effect whatever; but when it is necessary that the diameter of one of the shadows be increased, the corresponding cylinder is moved about its axis, till the wing just described emerging out of the shadow, and intercepting a portion of light, brings the shadow projected upon the field of the instrument to be of the width or diameter required. In this operation it is always necessary to turn the cylinder outwards, or in such a manner that the augmentation of the width of the shadow may take place on that side of it which is opposite to the shadow corresponding to the other light. The necessity for that precaution will appear evident to any one who has a just idea of the instrument in question, and of the manner of making use of it.

It is by means of these wings attached to the cylinders that the widths of the shadows are augmented, so as to fill the whole field of the *photometer*, when the screen with the circular opening is made use of.

As the lower ends of the cylinders which pass through the holes made to receive them in the bottom of the box are about $\frac{1}{20}$ of an inch less in diameter than their upper parts, which cast the shadows, and as they not only go quite through the bottom of the box (which is an inch thick) but project near an inch below its inferior surface, and, lastly, as these cylinders are not firmly fixed in these holes, it is easy, by taking hold of the ends of them which project below the bottom of the box, to turn about the cylinders upon their axes, even without opening the box. I said above, that the height of the vertical wing attached to each of the cylinders was equal to the height of the cylinder itself:—this must be understood to mean, not the total length of the cylinder, comprehending that part of it which passes into, and through the bottom of the box; but merely its height above the bottom of the box, or part projecting, namely $2\frac{1}{10}$ inches.

As it is absolutely necessary that the cylinders should constantly remain precisely perpendicular to the bottom of the box, or parallel to each other, it will be best to construct them of brass, and instead of fixing them immediately to the bottom of the box (which being of wood may warp), to fix them to a strong, thick piece of well hammered plate brass, which plate of brass may be afterwards fastened to the bottom of the box by means of one strong screw. In this manner two of my best instruments are constructed. And, in order to secure the cylinders still more firmly in their vertical positions, they are furnished with broad flat rings, or projections, where they rest upon the brass plate; which rings are $\frac{1}{10}$ of an inch thick, and equal in diameter to the projection of the wing of the cylinder, to the bottom of which they afford a firm support. These cylinders are likewise forcibly pushed, or rather pulled against the brass plate upon which they rest, by means of compressed spiral springs, placed between the under side of that plate and the lower ends of the cylinders.

Of whatever material the cylinders be constructed, and whatever be their forms or dimensions, it is absolutely necessary that they, as well as every other part of the photometer, except the field, should be well painted of a deep black, dead colour. That will prevent the inconveniences which would otherwise arise from reflected light, and from the presence of too great a number of visible objects.

In order to move the lights to and from the photometer with greater ease and precision, I have provided two long and narrow, but very strong and steady tables, in the middle of each of which there is a straight groove, in which a sliding carriage, upon which the light is placed, is drawn along by means of a cord which is fastened to it before and behind, and which passing over pullies at each end of the table, goes round a cylinder, which cylinder is furnished with a winch, and is so placed near the end of the table adjoining the photometer, that the observer can turn it about, without taking his eye from the field of the instrument.

Many advantages are derived from this arrangement; as first, the observer can move the lights as he finds necessary, without the help of an assistant, and even without removing his eye from the shadows; secondly, each light is always precisely in the line of direction in which it ought to be, in order that the shadows may be in contact in the middle of the vertical plane of the photometer; and thirdly, the sliding motion of the lights being perfectly soft and gentle, that motion produces little or no effect upon the lights themselves, either to increase or diminish their brilliancy.

These tables, which are 10 inches wide and 35 inches high, and the one of them 12 feet, and the other 20 feet long, are placed at an angle of 60° from each other, and in such a situation with respect to the photometer, that lines drawn through their middles in the direction of their lengths, meet in a point exactly under the middle of the vertical plane or field of the photometer, and from that point the distances of the lights are measured; the sides of the tables being divided into English inches, and a Vernier, showing tenths of inches, being fixed to each of the sliding carriages upon which the lights are placed.

These carriages are so contrived that they can be raised or lowered at pleasure, which is absolutely necessary in order that the lights may be always of a proper height, namely, that they may be in a horizontal line with the tops of the cylinders of the photometer.

The method of ascertaining when the lights are at the proper height has already been described.

In order that the two long and narrow tables, or platforms just described, upon which the lights move, may remain immoveable in their proper positions, they are both firmly fixed to the stand which supports the photometer; and in order that the motion of the carriages which carry the lights may be as soft and gentle as possible, they are made to slide upon parallel brass wires, 9 inches asunder, about $\frac{1}{10}$ of an inch in diameter, and well polished, which are stretched out upon the tables from one end to the other.

The pane of glass covered with white paper, which being fixed in a groove in the back of the box, constitutes the vertical plane upon which the shadows are projected, is $5\frac{1}{2}$ inches long, and $3\frac{1}{4}$ inches wide, as has already been observed; which is much larger than the dimensions assigned above for the field; namely, $1\frac{7}{10}$ inches wide, and 2 inches high. I had two objects in view in this arrangement; first, to render it easier to fix this plane in its proper position; and secondly, to be able to augment occasionally the dimensions of the field, by removing entirely the black pasteboard screen from before this plane, or making use of another with a larger aperture; which is sometimes advantageous.*

Having now, as I imagine, sufficiently described all the essential parts of these instruments, it remains for me to give some account of the precautions which, from experience, I have found it necessary to take in making use of them.

And first, with respect to the distance at which lights whose intensities are to be compared should be placed from the field of the photometer, I have found that when the weakest of the lights in question is about as strong as a common wax-candle, that light may most advantageously be placed from 30 to 36 inches from the centre of the field; and when it is weaker or stronger, proportionally nearer or farther off. When the lights are too near, the shadows will not be well defined; and when they are too far off, they will be too weak.

It will greatly facilitate the calculations necessary in drawing conclusions from experiments of this kind, if some steady light of a proper degree of strength for that purpose be assumed as a standard, by which all others may be compared. I have chosen for that purpose an ARGAND's lamp, made in London, and very well finished; and though the quantity of light emitted by this, or any other kind of lamp, is very various, depending in a great measure upon the length to which the wick is drawn out, yet I have found by repeated trials that this lamp, once properly adjusted, continues to emit light more equally for a considerable time than any other lamp, and much more so than any candle whatever.

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* Since writing the above, I have made a little alteration in the form of the box which contains my photometer. The front of it, instead of being open, is now closed, and the light is admitted through two horizontal tubes, which are placed so as to form an angle of 60°; their axes meeting at the centre of the field of the instrument. The field of the photometer is viewed through an opening made for that purpose in the middle of the front of the box, between the two tubes abovementioned. The annexed figures, Tab. X. XI. XII. XIII. will serve to give a clearer idea of the instrument, as it now is, in its most improved state.

At the beginning of each experiment I adjust this standard light in the following manner. Having placed the lamp upon its carriage, at the distance of 100 inches from the centre of the field of the photometer, measuring from the centre of the circular flame of the lamp, a cylindric wax-candle, of known weight and dimensions, and which is kept merely for that purpose, being lighted and trimmed, and made to burn with the greatest possible degree of brilliancy, is placed over against it, at a certain given distance (33 inches), and then the wick of the lamp is drawn out, or shortened, as it is found necessary, till the shadows corresponding to the lamp and to the candle, are precisely of the same density; this done, the proof candle is extinguished, and laid by for further use, and the projected experiment is immediately commenced.

Here the proof candle is, properly speaking, the standard, but the lamp is to be preferred to it for the experiments, on account of the superior constancy or equality of its light.

The only danger of error in this matter arises from the difficulty of procuring proof candles, which shall always give precisely the same quantity of light, or of making the same candle burn with exactly the same brilliancy at different times; I flattered myself at one time that even this cause of error and uncertainty, however insurmountable the difficulty appears, might be in a great measure removed. I conceived that if the light from the standard lamp and that of the proof candle, brought to be of the same intensity at the surface of the vertical plane, were really stronger at one time than at another, the equal shadows of the cylinders would be proportionally deeper, and that by comparing, at different times, the density of those shadows with a painted scale of shades, regularly graduated, any difference in the intensity of the standard light might be discovered and compensated; but upon making the experiment I found, what indeed a little patient reflection would have enabled me to foresee, that the apparent density of the two equal shadows corresponding to the lights compared with a painted scale of shades, *exposed in the same light*, is ever the same, however the intensity of the rays at the surface upon which those shadows are projected may vary.

There is however another method by which I think it probable that the standard lamp may be adjusted with the requisite degree of precision. It appears, from a considerable number of experiments, of which I shall hereafter give a more particular account, that the quantity of light emitted by a lamp which burns in the same manner, with a clear flame, *and without smoke*, is in all cases as the quantity of oil consumed. If, therefore, the standard lamp be so adjusted as always to consume a certain given quantity of oil in a given time, there is much reason to suppose that it may then be depended on as a just standard of light.

In order to abridge the calculations necessary in these inquiries, it will always be advantageous to place the standard lamp at the distance of 100 inches of the photometer, and to assume the intensity of its light at its source equal to unity; in this case (calling this standard light A, the intensity of the light at its source $= x = 1$; and the distance of the lamp from

from

the field of the photometer $= m = 100$;) the intensity of the illumination at the field of the photometer ($= \frac{x}{m^2}$[*]) will be expressed by the fraction $\frac{1}{100^2} = \frac{1}{10000}$; and the relative intensity of any other light which is compared with it, according to the directions before given, may be found by the following proportion: calling this light B, putting $y =$ its intensity at its source, and $n =$ its distance from the field of the photometer, expressed in English inches, as it is $\frac{y}{n^2} = \frac{x}{m^2}$ as was shewn in my former letter, or instead of $\frac{x}{m^2}$, writing its value $= \frac{1}{10000}$ it will be $\frac{y}{n^2} = \frac{1}{10000}$, and consequently y is to 1 as n^2 is to 10000; or the intensity of the light B at its source, is to the intensity of the standard light A at its source, as the square of the distance of the light B from the middle of the field of the instrument, expressed in inches, is to 10000; and hence it is $y = \frac{n^2}{10000}$.

Or, if the light of the sun or that of the moon be compared with the light of a given lamp or candle C, the result of such comparison may best be expressed in words, by saying, that the light of the celestial luminary in question, *at the surface of the earth*, or which is the same thing, at the field of the photometer, is equal to the light of the given lamp or candle, *at the distance found by the experiment*; or putting $a =$ the intensity of the light of this lamp C at its source, and $p =$ its distance in inches from the field, when the shadows corresponding to this light, and that corresponding to the celestial luminary in question, are found to be of equal densities; and putting $z =$ the intensity of the rays of the luminary at the surface of the earth, the result of the experiment may be expressed thus, $z = \frac{a}{p^2}$;

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[*] See my former letter.

or the real value of *a* being determined by a particular experiment, made expressly for that purpose with the standard lamp, that value may be written instead of it. When the standard lamp itself is made use of, instead of the lamp C, then the value of *a* will be 1.

I have been the more particular in this account of the instruments employed in these inquiries, the manner in which the experiments were conducted, and the principles upon which the conclusions drawn from them are founded, not only because the subject being new, the most particular information upon all these points is absolutely necessary, to enable others to judge with certainty of the matter submitted to their examination, but also because I was very desirous of affording every information and assistance in my power to those who may be disposed to prosecute these very curious and entertaining researches.

Hoping that this apology may be thought sufficient to excuse the prolixity of these descriptions, I shall now proceed to give a short account of such experiments as I have hitherto found leisure to make with this apparatus.

My first attempts were to determine how far it might be possible to ascertain, by direct experiments, the certainty of the assumed law of the diminution of the intensity of the light emitted by luminous bodies; namely, that the intensity of the light is every where as the squares of the distances from the luminous body inversely. These experiments appeared to me the more necessary, as it is quite evident that this law can only hold good when the light is propagated in perfectly transparent or unresisting spaces, or where, suffering no diminution whatever from the medium, its intensity is weakened merely in consequence of the divergency of the rays; and as it is more than probable that air, even in its purest state, is far from being perfectly transparent.

For greater perspicuity I shall arrange all my experiments and inquiries under general heads, and shall begin by prefixing to those which relate to the subject now under consideration, the general title of

Experiments upon the Resistance of the Air to Light.

EXPERIMENT I.

Two equal wax-candles, well trimmed, and which were found by a previous experiment to burn with exactly the same degree of brightness, were placed *together*, on one side, before the photometer, and their united light was counterbalanced by the light of an ARGAND's lamp, well trimmed, and burning very equally, placed on the other side over against them. The lamp was placed at the distance of 100 inches from the field of the photometer, and it was found that the two burning candles (which were placed as near together as possible, without their flames affecting each other by the currents of air they produced), were just able to counterbalance the light of the lamp at the field of the photometer, when they were placed at the distance of 60,8 inches from that field. One of the candles being now taken away and extinguished, the other was brought nearer to the field of the instrument, till its light was found to be just able, singly, to counterbalance the light of the lamp; and this was found to happen when it had arrived at the distance of 43,4 inches.

In this experiment, as the candles burnt with equal brightness, it is evident that the intensities of their united and single lights were as 2 to 1, and in that proportion ought, according to the assumed theory, the squares of the distances, 60,8 and 43,4 to be; and in fact, $\overline{60,8}^2 = 3696,64$ is to $\overline{43,4}^2 = 1883,56$ as 2 is to 1 very nearly.

Again, in another experiment, (No. 2.) the distances were,
with two candles = 54 inches. Square = 2916.
with one candle = 38,6. - = 1489,96

Upon another trial (experiment No. 3.)
with two candles = 54,6 inches. Square = 2981,16
with one candle = 39,7. - = 1576,09

And in the 4th experiment
with two candles = 58,4 inches. Square = 3410,56
with one candle = 42,2. - = 1780,84

And taking the mean of the results of these four experiments.

Squares of the distances.

		With two candles.		With one candle.
In the experiment	No. 1,	3696,64	-	1883,56
	No. 2,	2916	-	1489,96
	No. 3,	2981,16	-	1576,09
	No. 4,	3410,56	-	1780,84
		4) 13004,36	-	4) 6736,45
	Means	3251,09	and	1682,61,

which again are very nearly as 2 to 1.

With regard to these experiments it may be observed, that were the resistance of the air to light, or the diminution of the light from the imperfect transparency of air, sensible within the limits of the inconsiderable distances at which the candles were placed from the photometer, in that case the distance of the two equal lights united ought to be to the distance of one of them single, in a ratio less than that of the square root of 2 to the square root of 1. For if the intensity of a light emitted by a luminous body, *in a space void of all resistance*, be diminished in the proportion of the squares of the distances, it must of necessity be diminished in a still higher ratio when the light passes through a resisting medium, or one which is not perfectly transparent: and from the difference of those ratios, namely, that of the squares of the distances, and that other higher ratio found by the experiment, the resistance of the medium might be ascertained. This I have taken much pains to do with respect to air, but have not as yet succeeded in these endeavours, the transparency of air being so great that the diminution which light suffers in passing through a few inches, or even through several feet of it, is not sensible.

Having found, upon repeated trials, that the light of a lamp, properly trimmed, is incomparably more equal than that of a candle, whose wick continually growing longer renders its light extremely fluctuating, I substituted lamps to candles in these experiments, and made such other variations in the manner of conducting them, as I thought bid fair to lead to a discovery of the resistance of the air to light, were it possible to render that resistance sensible within the confined limits of my machinery.

Having provided two lamps, the one an ARGAND's lamp, which I made to burn with the greatest possible brilliancy; the other a small common lamp, with a single, round, and very small wick, which burning with a very clear, steady flame, and without any visible smoke, emitted only about $\frac{1}{25}$ part as much light as the ARGAND's lamp; these lamps being placed over against each other before the field of the photometer, their lights were found to be in equilibrium when the less being placed at the distance of 20 inches from the centre of that field, the greater was removed to the distance of 101 inches. I now concluded, that if the less light were to be removed to the distance of 40 inches, it would be necessary, in order to restore the equilibrium of light, or equality of the shadows in the field of the photometer, to remove the greater light to the distance of 202 inches; that is to say, if the diminution of the light arising from the imperfect transparency of the air should not be perceptible within the limits of that distance. But if, on the contrary, it should be found upon repeated trials, that the equilibrium was restored when the greater light had arrived at a distance *short* of 202 inches, I might thence conclude, that such effect might safely be attributed to the imperfect transparency of the air: for notwithstanding that the light of the smaller lamp would of course be diminished as well as that of the greater, yet as there is every reason to suppose that the diminution, whatever it may be, must ever be proportional to the distance through which the light passes in the medium, as the augmentation of the distance through which the light of the smaller lamp passes is no more than 20 inches, while that of the greater is made to pass through an additional distance, amounting to more than 100 inches, it is evident that the diminution of the light of the greater lamp, arising from the imperfect transparency of the medium, must be greater than the diminution of the light of the less lamp, arising from the same cause; and, consequently, that the effects of such diminution would become apparent in the experiment, were they in reality considerable.

The following table will shew the results of the experiments which were made with a view to determine that fact.

Experiments.	Distance of the smaller Light.	Distance of the greater Light.	Second distance of the greater light, *computed* according to the assumed law of the squares of the distances	Difference between the result of the experiment and the theory.
	inches.	inches.	inches.	inches.
No. 5.	First dist. 20	First dist. 101		
	Second dist. 40	Second dist. 203	202	+ 1
No. 6.	First dist. 20	First dist. 100,2		
	Second dist. 40	Second dist. 198,3	200,4	— 2,1
No. 7.	First dist. 20	First dist. 100,8		
	Second dist. 40	Second dist. 202,1	201,6	+ 0,5
No. 8.	First dist. 20	First dist. 101,5		
	Second dist. 40	Second dist. 204	203	+ 1
No. 9.	First dist. 50	First dist. 100		
	Second dist. 100	Second dist. 198	200	— 2
No. 10.	First dist. 50	First dist. 95,5		
	Second dist. 100	Second dist. 192,2	191	+ 1,2
No. 11.	First dist. 50	First dist. 95,1		
	Second dist. 100	Second dist. 191,2	190,2	+ 1
No. 12.	First dist. 50	First dist. 96		
	Second dist. 100	Second dist. 192,4	192	+ 0,4

In the four last experiments, instead of the small lamp above described, a common Argand lamp was made use of, the wick of which was only drawn out so far as to cause it to emit about ¼ part as much light as the other Argand's lamp, burning with its greatest brilliancy, which was placed over against it.

In order that in judging of the equality of the shadows, my mind might be totally unbiassed by my expectations, or by any opinions I might previously have formed with respect to the probable issue of the various experiments, keeping my eye constantly fixed upon the field of the photometer, and causing the light, whose corresponding shadow was to be brought to be of equal density with the standard, to move backwards and forwards, by means of the winch which I had constantly in my hand, as soon as the shadows appeared to me to be perfectly equal, I gave notice to an assistant to observe, and silently to write down, the distance of the lamp or candle, so that I did not even know what that distance was till the experiment was ended, and till it was too late to attempt to correct any supposed errors of my eyes by my wishes, or by my expectations, had I been weak enough to have had a wish in a matter of this kind. I do not know that any predilection I might have had for any favourite theory, would have been able to have operated so strongly upon my mind, and upon my senses, as to have made black and white appear to me otherwise than as they really were; but this I know, that I was very glad to find means to avoid being *led into temptation.*

But to return to the foregoing experiments; the results of them, so far from affording means for ascertaining the resistance of the air to light, do not even indicate any resistance at all; on the contrary, it might almost be inferred from some of them, that the intensity of the light emitted by a luminous body in air is diminished in a ratio *less* than that of the squares of the distances; but as such a conclusion would involve an evident absurdity, namely that, light moving in air, its absolute quantity instead of being diminished actually goes on to *increase*, that conclusion can by no means be admitted.

Besides the experiments above mentioned, I made a great number of others similar to them, and with the same view; but as their results were all nearly the same, I have not thought it worth while to lengthen my letter by inserting a particular account of them. In general they all conspired to shew that the resistance of the air to light was too inconsiderable to be perceptible; and that the assumed law of the diminution of the intensity of the light may with safety be depended on.

That the transparency of air in its purest state is very great, is evident from the very considerable distances at which objects, and such even as are but faintly illuminated, are visible; and I was by no means surprised that its want of transparency could not be rendered sensible in the small distance to which my experiments were necessarily confined: but still I think means may be found for rendering its resistance to light apparent, and even of subjecting that resistance to some tolerably accurate measure.

An accurate determination of the relative intensity of the sun's or moon's light, when seen at different heights above the horizon, or when seen from the top, and from the bottom of a very high mountain, in very clear weather, would probably lead to a discovery of the real amount of the resistance of the air to light.

Of the Loss of Light in its Passage through Plates or Panes of different Kinds of Glass.

In these experiments I proceeded in the following manner. Having provided two equal ARGAND's lamps, A and B, well trimmed, and burning with very clear bright flames, they were placed over against each other before the photometer, each at the distance of 100 inches from the field of the instrument, and the light of B was brought to be of the same intensity as that of A, or the shadows were brought to be of the same density, which was done by lengthening or shortening the wick of the lamp B, as the occasion required. This done, and the two lamps now burning with precisely the same degree of brilliancy, a pane of fine, clear, transparent, well polished glass, such as is commonly made use of in the construction of looking-glasses, six inches square, placed vertically upon a stand, in a small frame, was interposed before the lamp B, at the distance of about four feet from it, and in such a position that the light emitted by it was obliged to go perpendicularly through the middle of the pane, in order to arrive at the field of the photometer. The consequence of this was, that the light of the lamp B being diminished and weakened in its passage through the glass, the illuminations of the shadows in the field of the photometer were no longer equal, the shadow corresponding to the lamp A being now less enlightened by the light of the lamp B, than the shadow corresponding to the lamp B was enlightened by the undiminished light of the lamp A.

To determine precisely the exact amount of this diminution of the light of the lamp B (which was the main object of the experiment), nothing more was necessary than to bring this lamp nearer to the field of the photometer, till its light passing through the glass should be in equilibrium with the direct light of the lamp A; or, in other words, till the equality of the shadows should be restored; and this I found actually happened when the lamp B, from 100 inches, was brought to the distance of 90,2 inches from the field of the photometer.

Now as it has already been shewn that the intensities of the lights are as the squares of their distances from the field of the photometer, the illuminations being equal at that field, it is evident that the light of the lamp B was diminished, in this experiment, in its passage through the pane of glass, in the ratio of $\overline{100}^2$ to $\overline{90,2}^2$, or as 1 to ,8136; so that no more than ,8136 parts of the light which impinged against the glass found its way through it; the other ,1864 parts being dispersed and lost.

To assure myself that the lamps still continued to emit the same relative quantities of light as at the beginning of the experiment, I now removed the pane of glass, and found that the equality of the shadows was again restored, when the lamp B arrived at its former station, 100 inches from the field of the photometer.

This experiment I repeated no less than 10 times, and found the loss of light in its passage through this pane of glass, taking a mean of all the experiments, to be ,1973 parts of the whole quantity that impinged against it; the variations in the results of the various experiments being from ,1720 to ,2108.

In four experiments, with another pane of the same kind of glass, the loss of light was ,1836; ,1732; ,2056; and ,1853; mean ,1869.

When the two panes of this glass were placed before the lamp B, at the same time, but without touching each other, and the light made to pass through them both, the loss of light, in four different experiments, was ,3089 ; ,3259 ; ,3209 ; and ,3180 ; mean ,3184.

With another pane of glass of the same kind, but a little thinner, the mean loss of light, in four experiments, was ,1813.

With a very thin clean, pane of clear, white, or colourless window-glass, not ground, the loss of light, in four experiments, was ,1324 ; ,1218 ; ,1213 ; and ,1297 ; mean ,1263. When the experiment was made with this same pane of glass, a very little dirty, the loss of light was more than doubled.

Might not this apparatus be very usefully employed by the optician, to determine the degree of transparency of the glass he employs, and direct his choice in the provision of that important article in his trade ?

In making these experiments, a great deal of the trouble may well be spared, for there is no use whatever in bringing the two lamps A and B to burn with the same degree of brilliancy ; all that is necessary being to bring the shadows to be of the same density, with the glass, and without it, noting the distance of the lamp B in each case (the lamp A remaining immoveable in its place) ; for the relative quantity of light lost will ever be accurately shewn by the ratio of the squares of those distances, whatever be the relative brilliancy with which the two lamps burn. The experiment is more striking, and the consequences drawn from it rather more obvious, when the lamps are made to burn with equal flames ; otherwise that equality is of no real advantage.

Of the Loss of Light in its Reflection from the Surface of a plane Glass Mirror.

In these experiments the method of proceeding was much the same as in those just mentioned. The lamps A and B burning with clear, bright, and steady flames, were placed before the field of the photometer, and one of them was moved backwards and forwards till the illuminations of the shadows in the field of the instrument were found to be precisely equal. The distance of the lamp B being then noted, this lamp was removed, and a mirror being put in its place, but nearer the field of the photometer, the lamp was so placed that its rays, striking the centre of the mirror, were reflected against the field of the photometer, where, by bringing the lamp nearer to, or removing it farther from the mirror, the illumination of the field by those reflected rays was now brought to be in equilibrium with the illumination of the standard lamp, and then the distance of the lamp from the centre of the mirror, and the distance from thence to the centre of the field, were carefully measured, and noted. These two distances added together, was the real distance through which the rays passed in order to arrive at the field of the photometer.

Now as there is always a loss of light in reflection, it is evident that the reflected rays must come to the field of the photometer weakened, and that in order to illuminate this field by these reflected rays as strongly as it was illuminated by the direct rays of the same lamp, the lamp must be brought nearer to the field. It is likewise evident, from what has already been said, that the ratio of the squares of those distances of the lamp when its rays pass on directly, and when they arrive after having been reflected, are found to illuminate equally the field of the photometer, will be an accurate measure of the loss of the light in reflection.

The following table will shew the results of five experiments with a small, but most excellent glass mirror, made by RAMSDEN. This mirror, which makes part of an optical instrument I caused to be constructed in London about twelve years ago, is 7 inches long, and $5\frac{1}{2}$ inches wide, and I suppose is as perfect as ever glass mirror was of that size.

To facilitate the comparison of the results of the experiments, the lamp B, at the beginning of each experiment (when the intensity of its direct rays was compared with the intensity of the standard lamp), was placed at the distance of 100 inches, the standard lamp being occasionally moved, in order to produce an equality of the shadows.

Experiments.	The angle of incidence.	Distance of the centre of the mirror from the centre of the field.	Distance of the lamp from the centre of the mirror.	Real distance of the lamp, or length of the reflected rays.	Light lost in the reflection.
		inches.	inches.	inches.	parts.
1	60°	40	40,8	80,8	,3472
2	85°	—	41,	81,	,3439
3	45°	—	41,5	81,5	,3358
4	60°	—	39·5	79·5	,3680
5	70°	—	40,5	80,5	,3520

The mean of these five experiments gives for the loss of light ,3494 ; and from hence it appears, that more than $\frac{1}{3}$ part of the light which falls upon the best glass mirror that can be constructed is lost in reflection.

The loss with mirrors of indifferent quality, is still more considerable. With a very bad common looking-glass the loss, in one experiment, appeared to be ,4816 parts; and with another looking-glass it was ,4548 parts in one experiment, and ,4430 in another. I should certainly have made an experiment to determine the loss of light in its reflection from the surface of a plane metallic mirror, but I had no such mirror at hand.

The difference of the angles of incidence at the surface of the mirror, within the limits mentioned, namely, from 45° to 85°, did not appear to affect, in any sensible degree, the results of the experiments. I also found upon trial, that the effect produced by the difference of the angles at which light impinges against a sheet of transparent glass through which it passes, is, within the limits of 40° or 50° from the perpendicular, but very trifling.

Of the relative Quantities of Oil consumed, and of Light emitted, by an ARGAND'S *Lamp, and by a Lamp on the common Construction, with a Riband Wick.*

The brilliancy of the ARGAND'S lamp is not only unrivalled, but the invention is in the highest degree ingenious, and the instrument useful for many purposes; but still, to judge of its real merits as an illuminator, it was necessary to know whether it gives more light than another lamp *in proportion to the oil consumed.* This point I determined in the following manner.

Having placed an ARGAND'S lamp, well trimmed, and burning with its greatest brilliancy, before my photometer, and over against it a very excellent common lamp, with a riband wick, about an inch wide, and which burnt with a clear, bright flame, without the least appearance of smoke, I found the intensities of the light emitted by the two lamps to be to each other as 17956 to 9063; the densities of the shadows being equal when the Argand's being placed at the distance of 134 inches, the common lamp was placed at the distance of 95,2 inches, from the field of the photometer.

Both lamps having been very exactly weighed when they were lighted, they were now (without being removed from their places before the photometer), caused to burn with the same brilliancy just 30 minutes; when they were extinguished and weighed again, and were found to have consumed of oil, the Argand's lamp $\frac{253}{8192}$, and the common lamp $\frac{163}{8192}$ of a Bavarian pound.

Now as the quantity of light produced by the Argand's lamp, in this experiment, is to the quantity produced by the common lamp, as 17956 to 9063, or as 187 to 100; while the quantity of oil consumed by the former is to that consumed by the latter only in the ratio of 253 to 163, or as 155 to 100, it is evident that the quantity of light produced by the combustion of a given quantity of oil in an Argand's lamp is greater than that produced by burning the same quantity in a common lamp, in the ratio of 187 to 155, or as 100 to 85.

The saving, therefore, of oil which arises from making use of an Argand's lamp instead of a common lamp in the production of light, is evident; and it appears from this experiment that that saving cannot amount to less than 15 *per cent.* How far the advantage of this saving may, under certain circumstances, be counterbalanced by inconveniences that may attend the making use of this improved lamp, I will not pretend to determine.

Of the relative Quantities of Light emitted by an ARGAND'S *Lamp, and by a common Wax Candle.*

I have made a considerable number of experiments to determine this point, and the general result of them is, that a common ARGAND'S lamp, burning with its usual brightness, gives about as much light as nine good wax candles; but the sizes and qualities of candles are so various, and the light produced by the same candle so fluctuating, that it is very difficult to ascertain, with any kind of precision, what a common wax candle is, or how much light it ought to give. I once found that my ARGAND'S lamp, when it was burning with its greatest brilliancy, gave twelve times as much light as a good wax candle $\frac{3}{4}$ of an inch in diameter, but never more.

Of the Fluctuations of the Light emitted by Candles.

To determine to what the ordinary variations in the quantity of light emitted by a common wax candle might amount, I took such a candle, and lighting it, placed it before the photometer, and over against it an ARGAND'S lamp, which was burning with a very steady flame; and measuring the intensity of the light emitted by the candle from time to time, during an hour, the candle being occasionally snuffed when it appeared to stand in need of it, its light was found to vary from 100 to about 60. The light of a wax candle of an inferior quality was still more unequal, but even this was but trifling compared to the inequalities of the light of a tallow candle.

An ordinary tallow candle, of rather an inferior quality, having been just snuffed, and burning with its greatest brilliancy, its light was as 100; in eleven minutes it was but 39; after eight minutes more had elapsed, its light was reduced to 23; and in ten minutes more, or twenty-nine minutes after it had been last snuffed, its light was reduced to 16. Upon being again snuffed it recovered its original brilliancy, 100.

Of the relative Quantities of Bees Wax, Tallow, Olive Oil, Rape Oil, and Linseed Oil, consumed in the Production of Light.

In order to ascertain the relative quantities of bees wax and of olive oil consumed in the production of light, I proceeded in the following manner. Having provided an end of a wax candle of the best quality, ,68 of an inch in diameter, and about four inches in length, and a lamp with five small wicks, which I had found upon trial to give the same quantity of light as the candle, I weighed very exactly the candle, and the lamp filled with oil, and then placing them at equal distances (40 inches) before the field of the photometer, I lighted them both at the same time; and after having caused them to burn with precisely the same degree of brightness *just one complete hour*, I extinguished them both, and weighing them a second time, I found that 100 parts of wax, and 129 parts of oil, had been consumed.

Hence it appears, that the consumption of bees wax is to the consumption of olive oil, in the production of the same given quantity of light, as 100 is to 129.

In this experiment no circumstance was neglected that could tend to render the result of it conclusive. Care was taken to snuff the candle very often with a pair of sharp scissars, in order to make it burn constantly with the same degree of brilliancy;

and the light of the lamp was, during the whole time, kept in the most exact equilibrium with the light of the candle; which was easily done by occasionally drawing out, a little more or less, one or more of its five equal wicks. These wicks, which were placed in a right line perpendicular to a line drawn from the middle wick to the middle of the field of the photometer, were about $\frac{1}{10}$ of an inch in diameter each, and $\frac{1}{4}$ of an inch from each other, and when they were lighted, their flames united into one broad, thin, and very clear, white flame, without the least appearance of smoke.

In order to ascertain the relative consumption of olive oil and rape oil, in the production of light, two lamps, like that just described, were made use of; and the experiment being made with all possible care, the consumption of olive oil appeared to be to that of rape oil, in the production of the same quantity of light, as 129 is to 125.

The experiment being afterwards repeated with olive oil and very pure linseed oil, the consumption of olive oil appeared to be to that of the linseed oil as 129 to 120.

The experiment being twice made with olive oil, and with a tallow candle; once when the candle, by being often snuffed, was made to burn constantly with the greatest possible brilliancy, and once when it was suffered to burn the whole time with a very dim light, owing to the want of snuffing, the results of these experiments were very remarkable.

When the candle burnt with a clear, bright flame, the consumption of the olive oil was to the consumption of the tallow as 129 is to 101; but when the candle burnt with a dim light, the consumption of the olive oil was to the consumption of the tallow as 129 is to 229. So that it appeared from this last experiment, that the tallow, instead of being nearly as productive of light in its combustion as bees wax, as it appeared to be when the candle was kept constantly well snuffed, was now, when the candle was suffered to burn with a dim light, by far less so than oil.

But this is not all; what is still more extraordinary is, that the very same candle, burning with a long wick, and a dim light, actually consumed *more tallow* than when, being properly snuffed, it burnt with a clear, bright flame, and gave near *three times as much light!*

To be enabled to judge of the relative quantities of light actually produced by the candle in the two experiments, it will suffice to know, that in order to counterbalance this light at the field of the photometer, it required, in the former experiment, the consumption of 141 parts, but in the latter only the consumption of 64 parts of olive oil. But in the former experiment 110 parts, and in the latter 114 parts of tallow were actually found to be consumed. These parts were 8192ths of a Bavarian pound.

From the results of all the foregoing experiments it appears, that the relative expence of the undermentioned inflammable substances, in the production of light, is as follows.

		Equal parts in weight.
Bees wax.	A good wax candle, kept well snuffed, and burning with a clear, bright flame, - - -	100
Tallow.	A good tallow candle, kept well snuffed, and burning with a bright flame,	101
	The same tallow candle, burning very dim for want of snuffing, -	229

		Equal parts in weight.
Olive oil.	Burnt in an Argand's lamp, -	110
	The same burnt in a common lamp, with a clear, bright flame, without smoke, - - -	129
Rape oil.	Burnt in the same manner, -	125
Linseed oil.	Likewise burnt in the same manner,	120

I should have been very glad to have made the experiment with whale oil, but there was none to be had in the country I inhabit.

With the foregoing table, and the prices current of the therein mentioned articles, the relative prices of light produced by those different materials may very readily be computed.

The light of a wax candle, for instance, costs just *nine times* more at Munich, than the same quantity of light produced by burning rape oil in an Argand's lamp.

Of the Transparency of Flame.

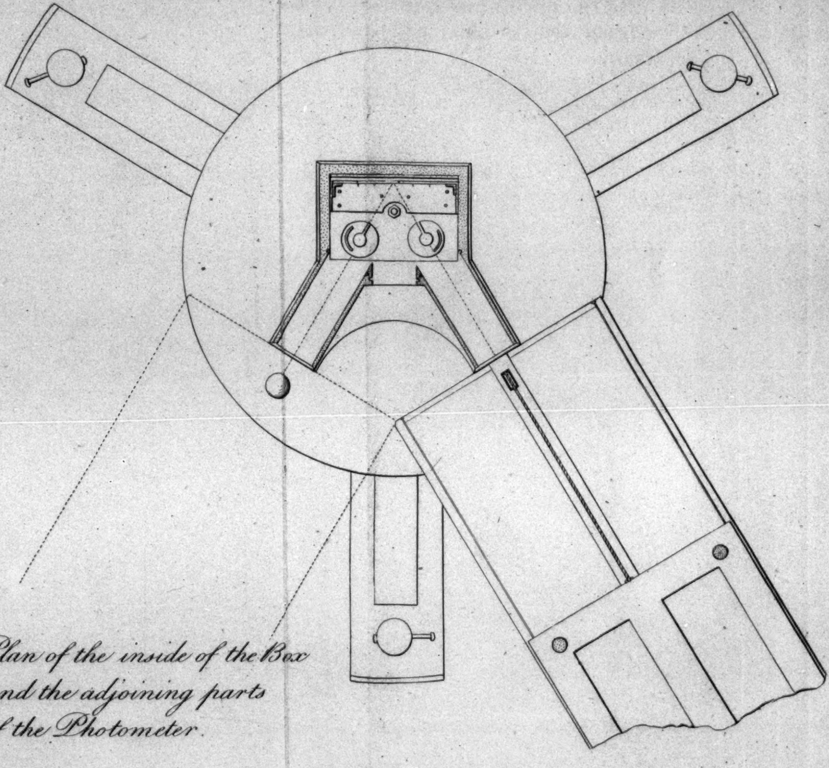
To ascertain the transparency of flame, or the measure of the resistance it opposes to the passage of foreign or extraneous light through it, I placed before the photometer, over against the standard lamp, two burning wax candles, well trimmed; and putting them near together, sometimes by the sides of each other, and sometimes in a straight line behind each other, I found that when their distances from the field of the photometer were the same, the intensity of the illumination was to all appearance the same, whether the light of the one was made to pass through the flame of the other, or not. And the same held good, with very little variation, when three, and even when four candles were made use of in the experiment, instead of two.

I even caused a lamp to be constructed with nine round wicks, placed in an horizontal line, and just so far asunder as to prevent their flames uniting, and no farther. And I found, upon repeating the experiment with this lamp, that the result was much the same as with the candles; the intensity of the illumination at the field of the photometer being very nearly the same, whether these nine lights were placed so as to cover, and pass through each other, or not.

But I afterwards found means to demonstrate the very great transparency of flame by a still more simple experiment. Suspecting that the only reason why bodies are not visible through a sheet of vivid flame is, that the light of the flame affects the eye in such a manner as to render it insensible to the weaker light emitted by, or reflected from the objects placed behind it, I conceived that a very strong light would not only be visible through a weak flame, but also (as all transparent bodies are invisible) that it might perhaps cause the flame totally to disappear; to determine that fact, I took a lighted candle at mid-day, the sun shining moderately bright, and holding it up between my eye and the sun, I found the flame of the candle to disappear entirely. It was not even necessary, in order to cause the flame to become invisible, to bring it to be directly between the eye and the body of the sun; it was sufficient for that purpose to bring it into the neighbourhood of the sun, where the light was very strong: even in a situation in which the light was not so strong as to dazzle the eye so much as to prevent its seeing very distinctly the body of the candle, and the wick, not the least appearance of flame was discernible, though the candle actually burnt the whole time very vigorously.

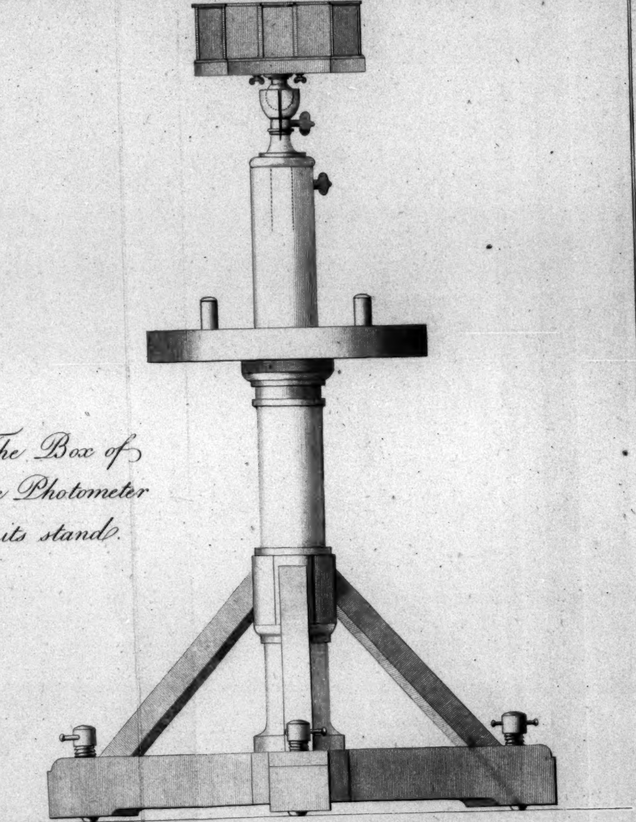
Munich,
1st March, 1793.

I am, &c.



Plan of the inside of the Box
and the adjoining parts
of the Photometer.

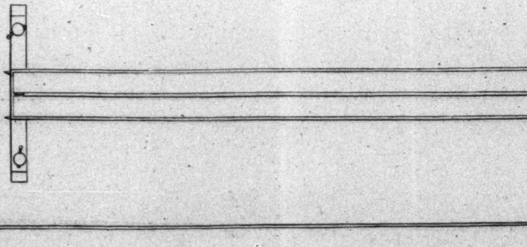
Basire Sc.



The Box of
the Photometer
on its stand.

Baſire ſc.

Plan of the two Tables belonging to the Photometer.



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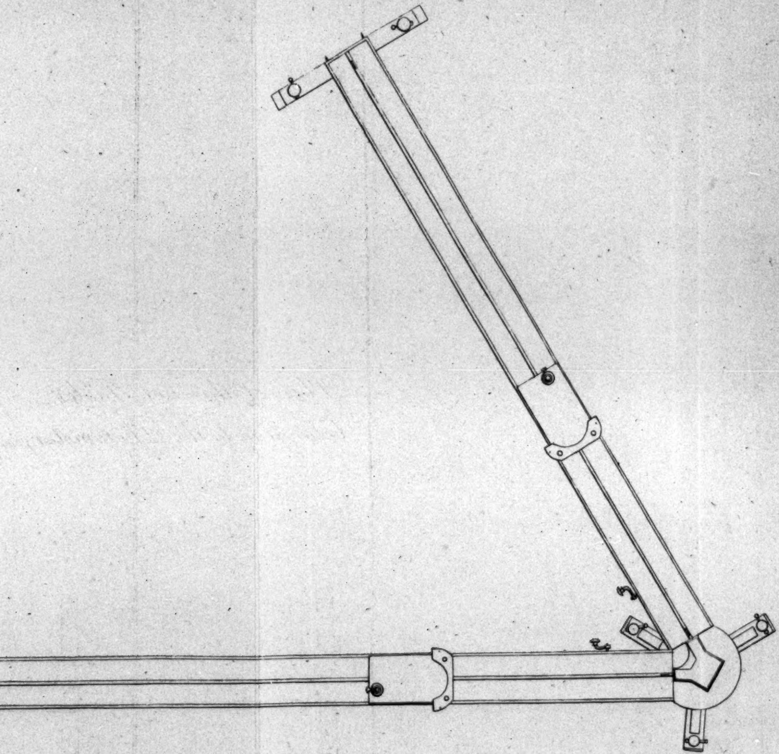

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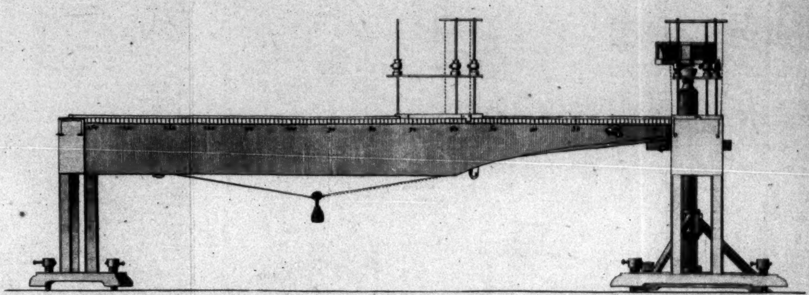

Elevation of the Photometer, with one of the Tables & Carriages.

Basire. Sc.

X. *An Account of some Experiments upon coloured Shadows. By Lieutenant-General Sir* Benjamin Thompson, *Count of* Rumford, *F. R. S. In a Letter to Sir* Joseph Banks, *Bart. P. R. S.*

Read February 20, 1794.

DEAR SIR,

SINCE my last letter, being employed in the prosecution of my experiments upon light, I was struck with a very beautiful, and what to me appeared to be a new appearance. Desirous of comparing the intensity of the light of a clear sky, by day, with that of a common wax candle, I darkened my room, and letting the daylight from the north, coming through a hole near the top of the window-shutter, fall at an angle of about 70° upon a sheet of very fine white paper, I placed a burning wax candle in such a position that its rays fell upon the same paper, and as near as I could guess, in the line of reflection of the rays of daylight from without; when interposing a cylinder of wood, about half an inch in diameter, before the centre of the paper, and at the distance of about two inches from its surface, I was much surprised to find that the two shadows projected by the cylinder upon the paper, instead of being merely shades without colour, as I expected, the one of them, that which, corresponding with the beam of daylight, was illuminated by the candle, was *yellow*; while the other, corresponding to the light of the candle, and consequently illuminated by the light of the heavens, was of the most beautiful *blue* that it is possible to imagine. This appearance, which was not only unexpected, but was really in itself in the highest degree striking and beautiful, I found, upon repeated trials, and after varying the experiment in every way I could think of, to be so perfectly permanent, that it is absolutely impossible to produce two shadows at the same time from the same body, the one answering to a beam of daylight, and the other to the light of a candle or lamp, without these shadows being coloured, the one *yellow* and the other *blue*.

The experiment may very easily be made at any time by day, and almost in any place, and even by a person not in the least degree versed in experimental researches. Nothing more is necessary for that purpose than to take a burning candle into a darkened room in the day time, and open one of the window-shutters a little, about half or three quarters of an inch for instance; when the candle being placed upon a table or stand, or given to an assistant to hold, in such a situation that the rays from the candle may meet those of daylight from without, at an angle of about 40°, at the surface of a sheet of white paper, held in a proper position to receive them, any solid opaque body, a cylinder, or even a finger, held before the paper, at the distance of two or three inches, will project two shadows upon the paper, the one blue, and the other yellow.

If the candle be brought nearer to the paper, the blue shadow will become of a deeper hue, and the yellow shadow will gradually grow fainter; but if it be removed farther off, the yellow shadow will become of a deeper colour, and the blue shadow will become fainter; and the candle remaining stationary in the same place, the same varieties in the strength of the tints of the coloured shadows may be produced merely by opening the window-shutter a little more or less, and rendering the illumination of the paper by the light from without stronger or weaker. By either of these means, the coloured shadows may be made to pass through all the gradations of shade, from the deepest to the lightest, and *vice versa*; and it is not a little amusing to see shadows, thus glowing with all the brilliancy of the purest and most intense prismatic colours, then passing suddenly through all the varieties of shade, preserving in all the most perfect purity of tint, growing stronger and fainter, and vanishing and returning at command.

With respect to the causes of the colours of these shadows, there is no doubt but they arise from the different qualities of the light by which they are illuminated; but how they are produced, does not appear to me so evident. That the shadow corresponding to the beam of daylight, which is illuminated by the yellow light of a candle, should be of a yellowish hue, is not surprising; but why is the shadow corresponding to the light of the candle, and which is illuminated by no other light than the apparently white light of the heavens, *blue*? I at first thought that it might arise from the blueness of the sky; but finding that the broad daylight, reflected from the roof of a neighbouring house covered with the whitest new fallen snow, produced the same blue colour, and if possible of a still more beautiful tint, I was obliged to abandon that opinion.

To ascertain with some degree of precision the real colour of the light emitted by a candle, I placed a lighted wax candle, well trimmed, in the open air, at mid-day, at a time when the ground was deeply covered with new fallen snow, and the heavens were overspread with white clouds; when the flame of the candle, far from being white, as it appears to be when viewed by night, was evidently of a very decided yellow colour, not even approaching to whiteness. The flame of an ARGAND's lamp, exposed at the same time in the open air, appeared to be of the same yellow hue. But the most striking manner of shewing the yellow hue of the light emitted by lamps and candles, is by exposing them in the direct rays of a bright meridian sun. In that situation the flame of an ARGAND's lamp, burning with its greatest brilliancy, appears in the form of a dead yellow semi-transparent smoke. How transcendently pure and inconceivably bright the rays of the sun are, when compared to the light of any of our artificial illuminators, may be gathered from the result of this experiment.

It appearing to me very probable, that the difference in the whiteness of the two kinds of light, which were the subjects of the foregoing experiments, might, some how or other, be the occasion of the different colours of the shadows, I attempted to produce the same effects by employing two artificial lights of different colours; and in this I succeeded completely.

In a room previously darkened, the light from two burning wax candles being made to fall upon the white paper at a proper angle, in order to form two distinct shadows of the cylinder, these shadows were found not to be in the least coloured; but upon interposing a pane of yellow glass, approaching to a faint orange colour, before one of the candles, one of the shadows immediately became *yellow*, and the other *blue*. When two ARGAND's lamps were made use of instead of the candles, the result was the same; the shadows were constantly and very deeply coloured, the one yellow approaching to orange, and the other blue approaching to green. I imagined that the greenish cast of this blue colour was owing either to the want of whiteness of the one light, or to the orange hue of the other, which it acquired from the glass.

When equal panes of the same yellow glass were interposed before *both* the lights, the white paper took an orange hue, but the shadows were to all appearance without the least tinge of colour; but *two* panes of the yellow glass being afterwards interposed before one of the lights, while only *one* pane remained before the other, the colours of the shadows immediately returned.

The result of these experiments having confirmed my suspicions, that the colours of the shadows arose from the different degrees of whiteness of the two lights, I now endeavoured, by bringing daylight to be of the same yellow tinge with candle light, by the interposition of sheets of coloured glass, to prevent the shadows being coloured when daylight and candle light were together the subjects of the experiment; and in this I succeeded. I was even able to reverse the colours of the shadows, by causing the daylight to be of a deeper yellow than the candle light. In the course of these experiments I observed, that different shades of yellow given to the daylight produced very different and often quite unexpected effects: thus one sheet of the yellow glass interposed before the beam of daylight, changed the yellow shadow to a lively violet colour, and the blue shadow to a light green; two sheets of the same glass nearly destroyed the colours of both the shadows; and three sheets changed the shadow which was originally yellow to blue, and that which was blue to a purplish yellow colour.

When the beam of daylight was made to pass through a sheet of blue glass, the colours of the shadows, the yellow as well as the blue, were improved and rendered in the highest degree clear and brilliant; but when the blue glass was placed before the candle, the colours of the shadows were very much impaired.

In order to see what would be the consequence of rendering the candle light of a still deeper yellow, I interposed before it a sheet of yellow or rather orange-coloured glass, when a very unexpected and most beautiful appearance took place; the colour of the yellow shadow was changed to orange, the blue shadow remained unchanged, and the whole surface of the paper appeared to be tinged of a most beautiful violet colour, approaching to a light crimson or pink; almost exactly the same hue as I have often observed the distant snowy mountains and valleys of the Alps to take about sunset. Is it not more than probable that this hue is in both cases produced by nearly the same combinations of coloured light? in the one case, it is the white snow illuminated at the same time by the purest light of the heavens, and by the deep yellow rays from the west; and in the other, it is the white paper illuminated by broad daylight, and by the rays from a burning candle, rendered still more yellow by being transmitted through the yellow glass. The beautiful violet colour which spreads itself over the surface of the paper will appear to the greatest advantage, if the pane of orange-coloured glass be held in such a manner before the candle, that only a part of the paper, half of it for instance, be affected by it, the other half of it remaining white.

To make these experiments with more convenience, the paper, which may be about 8 or 10 inches square, should be pasted or glued down upon a flat piece of board, furnished with a ball and socket upon the hinder side of it, and mounted upon a stand; and the cylinder should be fastened to a small arm of wood, or of metal, projecting forward from the bottom of the board for that purpose. A small stand, capable of being made higher or lower as the occasion requires, should likewise be provided for supporting the candle; and if the board with the paper fastened upon it be surrounded with a broad black frame, the experiments will be so much the more striking and beautiful. For still greater convenience, I have added two other stands, for holding the coloured glass through which the light is occasionally made to pass, in its way to the white surface upon which the shadows are projected. It will be hardly necessary to add, that in order to the experiments appearing to the greatest advantage, all light, which is not absolutely necessary to the experiment, must be carefully shut out.

Having fitted up a little apparatus according to the above directions, merely for the purpose of prosecuting these inquiries respecting the coloured shadows, I proceeded to make a great variety of experiments, some with pointed views, and others quite at random, and merely in hopes of making some accidental discovery that might lead to a knowledge of the causes of appearances which still seemed to me to be enveloped in much obscurity and uncertainty.

Having found that the shadows corresponding to two like wax candles were coloured, the one blue, and the other yellow, by interposing a sheet of yellow glass before one of them; I now tried what the effect would be when blue glass was made use of instead of yellow, and I found it to be the same; the shadows were still coloured, the one blue, and the other yellow, with the difference, however, that the colours of the shadows were reversed, that which, with the yellow glass, was before yellow being now blue, and that which was blue being yellow.

I afterwards tried a glass of a bright amethyst colour, and was surprised to find that the shadows still continued to be coloured blue and yellow. The yellow, it is true, had a dirty purple cast; but the blue, though a little inclining to green, was nevertheless a clean, bright, decided colour.

Having no other coloured glass at hand to push these particular inquiries farther, I now removed the candles, and opening two holes in the upper parts of the window-shutters of two neighbouring windows, I let into the room from above two beams of light from different parts of the heavens, and placing the instrument in such a manner that two distinct shadows were projected by the cylinder upon the paper, I was entertained by a succession of very amusing appearances. The shadows were tinged with an infinite variety of the most unexpected, and often most beautiful colours, which continually varying, sometimes slowly, and sometimes with inconceivable rapidity, absolutely fascinated the eyes, and commanding the most eager attention, afforded an enjoyment as new as it was bewitching. It was a windy day, with flying clouds, and it seemed as if every cloud that passed brought with it another complete succession of varying hues, and most *harmonious* tints. If any colours could be said to predominate it was purples; but all the varieties of browns, and almost all the other colours I ever remembered to have seen, appeared in their turns, and there were even colours which seemed to me to be perfectly new.

Reflecting upon the great variety of colours observed in these last experiments, many of which did not appear to have the least relation to the apparent colours of the light by which they were produced, I began to suspect that the colours of the shadows might, in many cases, notwithstanding their apparent brilliancy, be merely an optical deception, owing to contrast, or to some effect of the other neighbouring colours upon the eye. To determine this fact by a direct experiment, I proceeded in the following manner. Having, by making use of a flat ruler instead of the cylinder, contrived to render the shadows much broader, I shut out of the room every ray of daylight, and prepared to make the experiment with two ARGAND's lamps, well trimmed, and which were both made to burn with the greatest possible brilliancy; and having assured myself that the light they emitted was precisely of the same colour, by the shadows being perfectly colourless which were projected upon the white paper, I directed a tube about 12 inches long, and near an inch in diameter, lined with black paper, against the centre of one of the broad shadows; and looking through this tube with one eye, while the other was closed, I kept my attention fixed upon the shadow, while an assistant repeatedly interposed a sheet of yellow glass before the lamp whose light corresponded to the shadow I observed, and as often removed it. The result of the experiment was very striking, and fully confirmed my suspicions with respect to the fallacy of many of the appearances in the foregoing experiments. So far from being able to observe any change in the shadow upon which my eye was fixed, I was not able even to tell when the yellow glass was before the lamp, and when it was not; and though the assistant often exclaimed at the striking brilliancy and beauty of the blue colour of the very shadow I was observing, I could not discover in it the least appearance of any colour at all. But as soon as I removed my eye from the tube, and contemplated the shadow with all its neighbouring accompaniments, the other shadows rendered *really* yellow by the effect of the yellow glass, and the white paper which had likewise from the same cause acquired a yellowish hue, the shadow in question appeared to me, as it did to my assistant, of a beautiful blue colour. I afterwards repeated the same experiment with the apparently blue shadow produced in the experiment with daylight and candle-light, and with exactly the same result.

How far these experiments may enable us to account for the apparent blue colour of the sky, and the great variety of colours which frequently adorn the clouds, as also what other useful observations may be drawn from them, I leave to philosophers, opticians, and painters to determine. In the mean time, I believe it is a new discovery, at least it is undoubtedly a very extraordinary fact, that the eyes are not always to be believed, *even with respect to the presence or absence of colours.*

I cannot finish this letter without mentioning one circumstance, which struck me very forcibly in all these experiments upon coloured shadows, and that is, the most perfect harmony which always appeared to subsist between the colours, whatever they were, of the two shadows; and this harmony seemed to me to be full as perfect and pleasing when the shadows were of different tints of brown, as when one of them was blue and the other yellow. In short, the harmony of these colours was in all cases not only very striking, but the appearances were altogether quite enchanting; and I never found any body to whom I showed these experiments whose eyes were not fascinated with their bewitching beauties. It is, however, more than probable, that a great part of the pleasure which these experiments afforded to the spectators arose from the continual changes of colour, tint, and shade, with which the eye was amused, and the attention kept awake. We are used to seeing colours fixed and unalterable, hard as the solid bodies from which they come, and just as motionless, consequently dead, uninteresting, and tiresome to the eye; but in these experiments all is motion, life, and beauty.

It appears to me very probable, that a further prosecution of these experiments upon coloured shadows may not only lead to a knowledge of the real nature of the harmony of colours, or the peculiar circumstances upon which that harmony depends; but that it may also enable us to construct instruments for producing that harmony, for the entertainment of the eyes, in a manner similar to that in which the ears are entertained by musical sounds. I know that attempts have already been made for that purpose; but when I consider the means employed, I am not surprised that they did not succeed. Where the flowing tide, the varying swell, the *crescendo* is wanting, colours must ever remain hard, cold, and inanimate masses.

I am very sorry that my more serious occupations do not at present permit me to pursue these most entertaining inquiries. Perhaps at some future period I may find leisure to resume them.

Munich,
1st March, 1793.

I am, &c.

XI. *Investigations, founded on the Theory of Motion, for determining the Times of Vibration of Watch Balances.* By George Atwood, *Esq. F. R. S.*

Read February 27, 1794.

INSTRUMENTS for measuring time by vibratory [*] motion were invented early in the sixteenth [†] century: the single pendulum [‡] had been known to afford a very exact measure of time long before this period; yet it appears from the testimony of historical accounts, as well as other evidences, that the balance was universally adopted in the construction of the first clocks and watches; nor was it till the year 1657 that Mr. HUYGENS united pendulums with clock-work.

The first essays of an invention, formed on principles at once new and complicated, we may suppose were imperfectly executed. In the watches of the early constructions, some of which are still preserved, the balance vibrated merely by the impulses of the wheels, without other control or regulation: the motion communicated to the balance by one impulse continued till it was destroyed, partly by friction, and partly by a succeeding impulse in the opposite direction; the vibrations must of course have been very unsteady and irregular.

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[*] The ancients, as early as 140 years before CHRIST (probably much earlier) were acquainted with the use of wheel-work in constructing instruments for measuring time. "Denticuli alius alium impellentes, versationes modicas faciunt ac motiones," is the expression of VITRUVIUS in describing a machine, one of the principal uses of which was to indicate the hour of the day. Vibrations are no where mentioned or alluded to in the descriptions of the clocks constructed by the ancients. Dr. DERHAM on Clock-work, p. 86, 4th edit.

[†] About the year 1500, according to some accounts.

[‡] TYCHO BRAHE is supposed to have used the pendulum in astronomical observations. RICCIOLUS, KIRCHER, MERSENNUS, and many others, are expressly mentioned by STURMIUS to have employed this method of measuring time.

These imperfections were in a great measure remedied by Dr. Hooke's ingenious invention of applying a spiral spring to the balance: * the action of this spring on the balance of a watch, is similar to that of gravity on a pendulum: each kind of force has the effect of correcting the irregularities of impulse and resistance, which otherwise disturb the isochronism of the vibrations.

During the present century, various improvements have been made in the construction of watches, principally by the artists of this country, to whose ingenuity and skill, aided and encouraged by public rewards, we must attribute the excellence of the modern watches and time-keepers, so highly valuable for their uses in geography, navigation, and astronomy.

The principles on which time-keepers are constructed, considered in a theoretical view, afford an interesting subject of investigation. It is always satisfactory to compare the motion of machines with the general laws of mechanics, whenever friction and other irregular forces are so far diminished as to allow of a reference to theory; especially if inferences, likely to be of practical use, may be derived from such comparison.

* Anno 1658.—An inscription on a balance-spring watch, presented to King Charles II. fixes the date of this invention to the year 1658. Dr. Derham relates, that he had seen the watch, on which the following inscription was engraved: "Robert Hooke inven. 1658. T. Tompion fecit, 1675." Dr. Derham on Clock-work, p. 103.

In time-keepers, the irregular forces, both of impulse and resistance, are much diminished by the exactness of form and dimension which is given to each part of the work; and they are further corrected by the maintaining power derived from the main spring: for whatever motion is lost by the balance from resistance of any kind, almost the same motion is communicated by the maintaining power, so as to continue the arc of vibration, as nearly as possible, of the same length.

In these machines, the real measure of time is the balance, all the other work serving only to continue the motion of the balance, and to indicate the time as measured by its vibrations. The regularity of a time-keeper will therefore depend on that of the time in which the balance vibrates: to investigate this time of vibration, from the several data or conditions on which it depends, is the object of the ensuing pages.

Let PMNS (Tab. XIV. fig. 1.) represent the circumference of a watch balance, which vibrates by the action of a spiral* spring, on an axis passing through the centre C. Let ODBE be the circumference of a concentric circle, considered as fixed, to which the motion of the balance may be referred. In the circumference of this circle let any point O be assumed, and when the balance is in its quiescent position, suppose a line to be drawn through C and O, intersecting the circumference of the balance in the point A; the radius CA will be an index, by which the position of the balance, and its motion through any different arcs of vibration, will be truly defined. In the ensuing pages, the motion of the balance, and the motion of the index CA, will be used indifferently, as terms conveying the same meaning. Since the balance is in its quiescent position when the index C A is directed to the fixed point O, on this account O is called the point of quiescence of the balance, or balance spring, indicating the position when the balance is not impelled by the spring's elastic force either in one direction or the other. If the balance should be turned through any angle O C B, the spiral spring being wound through the same angle, endeavours by its elastic force to restore itself; and when at liberty, impels the balance through the arc B O with an accelerated velocity till it arrives at the position O, where the force of acceleration ceases; with the velocity acquired at O, the balance proceeds in its vibration, describing the arc O E with a retarded motion.

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* In these investigations it is indifferent whether the balance is supposed to vibrate by the action of a spiral or helical spring.

The elastic forces of the spring at equal distances on the opposite sides of the point O, are assumed to be equal; it is also assumed that the effects of friction, and other irregular resistances which retard the motion of the balance, are compensated by the maintaining power, so that the time of describing the first arc of vibration B O by an accelerated motion, shall be equal to the time of describing the latter arc O E by a retarded motion, and that the entire arc of vibration B O E is bisected by the point O.

To render the construction of fig. 1. more distinct, the fixed circle O D B E is represented to be at a small distance from the circumference of the balance, but is to be considered as coincident with it, so that the arc B O subtending the angle B C O, may be of the same length with an arc of the circumference of the balance which subtends the same angle B C O: on this principle C O or C A may be taken indifferently as the radius of the balance.

The determination of the time in which the balance vibrates, from the theory of motion, requires the following particulars to be known.

1st. The spring's elastic force, which impels the circumference of the balance when it is at a given angular distance O D (fig. 1.) from the quiescent point O.

2dly. The law or ratio observed in the variation of the spring's force, while the balance is impelled from the extremity of the semiarc B to the point of quiescence O, where all acceleration ceases.

3dly. The weight of the balance, including the parts which vibrate with it.

4thly. The radius of the balance C O, and the distance of the centre of gyration from the axis of motion C G.

5thly. The length of the semiarc B O.

Suppose the plane of the balance to be placed vertically, and let a weight P (fig. 2.) be applied by means of a line suspended freely from the circumference at T, to counterpoise the elastic force of the spring when the balance is wound through an angle from quiescence O C D. This weight P (the weight of the line being allowed for) will be the force of the spiral spring which impels the circumference of the balance, when at the angular distance O D, from the quiescent position.

It appears from many experiments, that the weights necessary to counterpoise a spiral spring's elastic force, when the balance is wound to the several distances from the quiescent point, represented* by the arcs OG, OH, OI, (fig. 2.) &c. are nearly in the ratio of those several arcs. It also appears, that the shape, the length, and number of turns of the spiral may be so adjusted to each other, that the forces of elasticity shall be counterpoised by weights which are in the precise ratio of the angular distances from the quiescent position, or, as it is sometimes expressed, in the ratio of the spring's tensions; at least as nearly as can be ascertained by experiment: this law of elastic force is assumed in the subsequent investigation.

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* Berthoud *Traité des Horloges marines*, p. 49.

The position of the centre of gyration may be always determined when the figure of the vibrating body is regular, by calculating the sum of the products which arise from multiplying each particle into the square of its distance from the axis of motion, and dividing the sum by the weight of the vibrating body; the square root of the result will be the distance of the centre of gyration from the axis of motion. When the figure of the vibrating body is irregular, recourse may be had to experimental * methods, in order to determine the position of the centre of gyration.

Let the radius of the balance CA or $CO = r$, (fig. 1.) the semiarc $BO = b$; let the spring's elastic force, acting on the circumference of the balance, when wound to any given angle OCD from the quiescent position be $= P$, and let the arc $OD = a$; the weight of the balance, and the parts which vibrate with it $= W$; the distance of the centre of gyration from the axis of motion $CG = g$. These notations being premised, the resistance of inertia by which the mass contained in the balance opposes the communication of motion to the circumference is $\frac{Wg^2}{r^2}$: and consequently the force which accelerates the circumference at the angular distance OCD from the quiescent position is $\frac{Pr^2}{Wg^2}$. This quantity remaining invariably the same, while the balance describes the arc of vibration BOE, may be denoted by the letter F, so that

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* Treatise on the Rectilinear Motion and Rotation of Bodies, p. 226 and 301.

$F = \frac{P r^2}{W g^2}$; suppose the radius CA commencing a vibration from the point B to have described the arc B H, and let O H $= x$; since the force which accelerates the circumference at the angular distance from quiescence O D is $= F$, and the forces of acceleration are supposed to vary in the proportion of the angular distances from the quiescent point O, the force which accelerates the circumference of the balance at the point H will be $= \frac{Fx}{a}$; let u be the space through which a body falls freely from rest by the acceleration of gravity to acquire the velocity of the circumference, at the point H; the principles of acceleration give this equation, * $\dot{u} = \frac{-Fx\dot{x}}{a}$;

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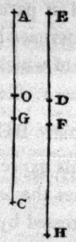
[*] NEWTONII Princ. Vol. I. prop. xxxix. Let a body describe the line AC by the acceleration of a force varying in any ratio of the distances from a centre C. Let another body describe the line E H by the acceleration of a constant or uniform force. Suppose the velocity at O to be equal to the velocity at D, and let O G and D F be the evanescent spaces, or increments of space in which equal velocities are generated; so that E D may represent a line through which a body must fall from rest by the acceleration of the constant or uniform force, to acquire the velocity of the other body at O. It is to be proved that the increment of space O G is to the increment of space D F, as the force of acceleration at D to the force of acceleration at O. Let the former of these forces, *i. e.* at D be denoted by G, and the latter force at O by H. Let E D $= u$, and let A O $= x$. Also let D F $= \dot{u}$, and O G $= \dot{x}$.



Because the increments of velocity are always as the forces of acceleration and the elementary times in which they act jointly, it follows, that when the increments of velocity are equal, the forces are in the inverse ratio of the elementary times in which they act; that is (the velocities of describing the evanescent spaces O G, D F being equal by the supposition), the forces are in the inverse ratio of those spaces; and consequently the force at D (G) is to the force at O (H) as O G to D F; that is, according to the preceding notation, $G : H :: \dot{x} : \dot{u}$ or $\dot{u} = \frac{H\dot{x}}{G}$. The constant force G being assumed equal to that of gravity, may be denoted by any constant quantity, such as unity. By substituting therefore 1 for G, the equation will become $\dot{u} = H\dot{x}$. In this and taking the fluents while x decreases from b to x, $u = \frac{F \times \overline{b^2 - x^2}}{2a}$: if therefore l is made $= 193$ inches, being the space which bodies falling freely from rest by the force of gravity near the earth's surface describe in one second of time, the velocity of the circumference, when the extremity A of the index C A has arrived at the point H, will be $= \sqrt{\frac{2lF}{a}} \times \sqrt{b^2 - x^2}$.

Let t represent the time in which the circumference describes the arc B H; then will $\dot{t} = \sqrt{\frac{a}{2lF}} \times \frac{-\dot{x}}{\sqrt{b^2 - x^2}}$; and $t = \sqrt{\frac{a}{2lF}} \times$ into a circular arc, of which the cosine $= \frac{x}{b}$ to radius $= 1$, which is the time of describing the arc B H expressed in parts of a second; when $x = 0$, that is when the circumference has described the entire semiarc B O, the circular arc of which the cosine $= \frac{x}{b}$ is a quadrant of a circle to radius $= 1$. Let $p = 3.14159$, &c. The time t, of describing the semiarc B O $= \sqrt{\frac{a}{2lF}} \times \frac{p}{2} = \sqrt{\frac{p^2 a}{8lF}}$.

In this expression for the time of a semivibration, the letter a denotes the length of the arc OD (fig. 1.); if this arc should be expressed by a number of degrees c°, a will then $= \frac{prc^\circ}{180^\circ}$; and this quantity being substituted for a, the time of a semivibration will be $t = \sqrt{\frac{p^3 r c^\circ}{8lF \times 180^\circ}}$; if instead of F, its value $\frac{Pr^2}{Wg^2}$ is substituted in the equation $t = \sqrt{\frac{p^3 r c^\circ}{8lF \times 180^\circ}}$, the time of a semivibration will be $t = \sqrt{\frac{Wp^3 g^2 c^\circ}{8Prl \times 180^\circ}}$.

Let the given arc c° be $= 90^\circ$; in this case $t = \sqrt{\frac{Wp^3 g^2}{16Prl}}$.

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expression the lines u and x are supposed to increase together; but if u increases while x decreases, the signs of the variations $\dot{u}$ and $\dot{x}$ will be contrary; in which case the equation will become $\dot{u} = -H\dot{x}$.

These are expressions for the time of a semivibration, whatever may be the figure of the balance, the other conditions remaining the same as they have been above stated. If the balance should be a cylindrical plate, it is known that the distance of the centre of gyration from the axis is to the radius as 1 to $\sqrt{2}$; wherefore in this case $g^2 = \frac{r^2}{2}$; and the time of a semivibration, or $t = \sqrt{\frac{W p^3 r}{32 P l}}$.*

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* The balances of watches are usually of such a form as to place the centre of gyration nearly at the same distance from the axis, as if the figures were cylindrical plates of uniform thickness and density. If it should be required to obtain from theory the time of a balance's vibration precisely exact, it would be necessary to calculate rigidly the position of the centre of gyration from the dimensions of each part of the balance, and whatever vibrates with it. But in cases merely illustrative of the general theorems for ascertaining the times of vibration, it is unnecessary to enter into prolix and troublesome calculations depending on the form of any particular balance; since by assuming it as a cylindrical plate, the time of a vibration will not differ materially from that which would be the result of the correct investigation.

Being desirous of comparing the time of vibration, as deduced from the theory of motion, with the actual vibration of a watch balance, I requested Mr. EARNSHAW (the excellent performance of whose time-keepers is well known) to make the experiments from which the necessary data for this calculation are derived. These experiments were made on the balance of a watch constructed by Mr. KENDAL, on Mr. HARRISON'S principles, and is the instrument which Captain COOK took out with him during his last voyage to the South Seas. The results are underneath:

Diameter of the balance - - - - - -	$2\frac{1}{4}$ inches.
Weight of the balance, and parts which vibrate with it - -	42 grains.
Weight applied to the circumference of the balance, which counterpoises the force of the spiral spring when the balance is wound through an angle of 180° - - - - - - -	48 grains.
The weight which counterpoises the spring's force when the balance is wound to 90 degrees from quiescence is - - -	24 grains.

These determinations give the following substitutions in the expression for the time of a semivibration $t = \sqrt{\frac{W p^3 r}{32 P l}}$.

It is observable that the semiarc of vibration B O $= b$, does not enter into these expressions for the time of a semivibration; if therefore instead of the semiarc B O, an arc of any other length LO, terminating in the point of quiescence O, (fig. 1.) should be substituted in the preceding investigation, the time of describing L O would be still $= \sqrt{\frac{a p^2}{8 l F}}$ or $\sqrt{\frac{p^3 r c^\circ}{8 l F \times 180^\circ}}$ equal to the time of describing the other semiarc B O; consequently, whether the balance vibrates in the largest or smallest arcs, the times of vibration will be the same.

From the preceding investigations it appears, that when the force by which the circumference of the balance is accelerated at the given angular distance c° from the quiescent position is $= F$, the time of a semivibration $t = \sqrt{\frac{p^3 r c^\circ}{8 l F \times 180^\circ}}$; and conversely, when the time of a semivibration is $= t$, the force which accelerates the circumference at the given angular distance c° from the quiescent position, that is $F = \frac{p^3 r c^\circ}{8 l t^2 \times 180^\circ}$.

Since watches and time-keepers are usually adjusted to

Namely, $W = 42$ gr. = the weight of the balance, including the parts which vibrate with it.

$P = 24$ gr. = the force at the circumference of the balance, which counterpoises the force of the spring when wound to the distance 90°.

$r = 1.125$ inch. and parts = the radius of the balance.

$l = 193$ inches = the space described in one second of time by bodies which descend freely from rest by the acceleration of gravity.

$p = 3.14159$, &c. = the circumference of a circle of which the diameter $= 1$;

pts. of a second.

the time of a semivibration $t = \sqrt{\frac{42 \times \overline{3.14159}^3 \times 1.125}{32 \times 24 \times 193}} =$ 0.0994

The balance, when adjusted to mean time, makes 5 vibrations in a second; the actual time of a semivibration is therefore - - 0.1000

Difference between the actual time and the time by the calculation - 0.0006 mean time when the balance makes 5 vibrations in a second, the time of a semivibration will in this case $= \frac{1}{10}$ part of a second: the substitution of $\frac{1}{10}$ for t being made in the preceding equation, the force which accelerates the circumference of the balance, when at any given angular distance c° from the quiescent position, will be determined for all time-keepers adjusted to mean time, in which the balances make 5 vibrations in a second. Suppose the given angle $c^\circ = 90^\circ$; then making $c^\circ = 90^\circ$, $p = 3.14159$, &c. $l = 193$, $t = \frac{1}{10}$, the accelerative force at the angular distance from quiescence 90° or $F = \frac{p^3\, r . 90^\circ}{8\, l\, t^2 \times 180^\circ} = r \times 1.00408926$. We have therefore arrived at the following conclusion: if the radius of the balance is equal to 1 inch, and the time-keeper is adjusted to mean time when the balance makes 5 vibrations in a second, the force which accelerates the circumference of the balance at the distance of 90° from the quiescent position, is $= 1.00408926$, the accelerative force of gravity being $= 1$. And if the radius of the balance is greater or less than 1 inch, the force by which the circumference is accelerated at the distance of 90° from quiescence, will be greater or less than 1.00408926 in proportion to the radii.

According to the principles assumed in the preceding solution, the spring's elastic force is supposed to vary in the proportion of the angular distances from the quiescent position, and on this condition, the vibrations are shewn to be isochronous, whether they are performed in longer or shorter arcs; but if the spring's elastic force at different distances from quiescence should not be precisely in the ratio here assumed, the longer and shorter arcs may be described in times differing in any proportions of inequality. If, for instance, the spring's force, instead of varying in the ratio of the aforesaid distances, should vary in the $\frac{999}{1000}$ power, or $\frac{1001}{1000}$ power of the distances, it does not appear from the preceding solution what alteration in the daily rate would be caused by this change in the law of the force's variation, when the semiarc of vibration is increased or diminished by a given arc. To ascertain this point fully, other researches will be necessary, by which it may be known, what alteration of the daily rate of a time-keeper is occasioned by a given increase or diminution of the arc of vibration, when the spring's elastic force varies in a ratio of the distances from the quiescent position, the general index or exponent of which is any number or fraction n.

The force which accelerates the balance being assumed in that power of the distances the exponent of which is n, let $BO = b$ (fig. 3.) be the semiarc of vibration when the time-keeper is adjusted to mean time; let $DO = a$; the accelerating force on the circumference at the distance from quiescence $OD = F$; suppose the circumference to have described the arc BH from the extremity of the arc B; and let $HO = x$: then the force by which the circumference is accelerated when at the angular distance from the quiescent position $OH = \frac{Fx^n}{a^n}$; let u be the space through which a body falls freely from rest by the acceleration of gravity, to acquire the velocity of the circumference when it has described the arc BH; the principles of acceleration give this equation: $\dot{u} = \frac{-Fx^n\dot{x}}{a^n}$: taking the fluents while x decreases from b to x, $u = \frac{Fb^{n+1} - Fx^{n+1}}{\overline{n+1} \times a^n}$, and l being 193 inches, the velocity acquired by the circumference after describing BH, will be

$$= \sqrt{\frac{4lF}{\overline{n+1}\times a^n}} \times \sqrt{b^{n+1} - x^{n+1}};$$

let T be the time of describing the arc BH; wherefore

$$\dot{T} = \sqrt{\frac{\overline{n+1}\times a^n}{4lF}} \times \frac{-\dot{x}}{\sqrt{b^{n+1} - x^{n+1}}}.$$

The time of describing the arc BH will be the fluent of this fluxion, while x decreases from b to x, and the time of describing the semiarc BO will be the entire fluent of $\sqrt{\frac{\overline{n+1}\times a^n}{4lF}} \times \frac{-\dot{x}}{\sqrt{b^{n+1} - x^{n+1}}}$, while x decreases from b to o.

Now let the balance commence its vibration from any other point I, (fig. 3.) and let IO $= c$; suppose the circumference to have described the arc IK, and make OK $= y$; let t be the time of describing the arc IK; then by proceeding in the same manner as in the former case, it is found that $\dot{t} = \sqrt{\frac{\overline{n+1}\times a^n}{4lF}} \times \frac{-\dot{y}}{\sqrt{c^{n+1} - y^{n+1}}}$; and the time of describing the semiarc IO, will be the entire fluent of this fluxion, while y decreases from c to o. Although the fluents of the fluxions $\frac{-\dot{x}}{\sqrt{b^{n+1} - x^{n+1}}}$ and $\frac{-\dot{y}}{\sqrt{c^{n+1} - y^{n+1}}}$ cannot be expressed in general terms, yet the exact proportion of the said fluents may be assigned, which will be the proportion of the times in which the balance vibrates in the two semiarcs BO, IO; the multiplying quantity $\sqrt{\frac{\overline{n+1}\times a^n}{4lF}}$ being common to both fluxions; and since the entire fluent[*] of $\frac{-\dot{x}}{\sqrt{b^{n+1} - x^{n+1}}}$ is to the entire fluent of

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[*] $\frac{\dot{x}}{\sqrt{b^{n+1} - x^{n+1}}} = \frac{1}{b^{\frac{n+1}{2}}} \times \frac{\dot{x}}{\sqrt{1 - \left.\overline{\tfrac{x}{b}}\right|^{n+1}}}$

$\frac{-\dot{y}}{\sqrt{c^{n+1}-y^{n+1}}}$ as $b^{\frac{1-n}{2}}$ is to $c^{\frac{1-n}{2}}$, it follows, that the time of a semivibration in the arc B O is to the time of a semivibration in the arc I O, as $b^{\frac{1-n}{2}}$ to $c^{\frac{1-n}{2}}$, or as 1 to $\left.\overline{\frac{IO}{BO}}\right|^{\frac{1-n}{2}}$.

Suppose a watch to be adjusted to mean time when the semiarc of the balance's vibration is = B O (fig. 3.) and let this semiarc be afterwards diminished to I O; the time shewn by the watch in any given portion of mean time *t*, when the semiarc of vibration is I O, will be $= t \times \left.\overline{\frac{BO}{IO}}\right|^{\frac{1-n}{2}}$; and if *t* is put = 24^{h}, the alteration of the daily rate, in consequence of the dimi-

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and $\sqrt{\frac{\dot{y}}{c^{n+1}-y^{n+1}}} = \frac{1}{c^{\frac{n+1}{2}}} \times \sqrt{\frac{\dot{y}}{1-\left.\overline{\frac{y}{c}}\right|^{n+1}}}$.

To find the proportion of the entire fluent of $\sqrt{\frac{\dot{x}}{1-\left.\overline{\frac{x}{b}}\right|^{n+1}}}$

to the entire fluent of $\sqrt{\frac{\dot{y}}{1-\left.\overline{\frac{y}{c}}\right|^{n+1}}}$

make $x = \frac{by}{c}$, so that when $x = 0$, $y = 0$, and when $x = b$, $y = c$;

then $\sqrt{\frac{\dot{x}}{1-\left.\overline{\frac{x}{b}}\right|^{n+1}}} = \frac{b}{c} \times \sqrt{\frac{\dot{y}}{1-\left.\overline{\frac{y}{c}}\right|^{n+1}}}$,

and the proportion of $\sqrt{\frac{\dot{x}}{1-\left.\overline{\frac{x}{b}}\right|^{n+1}}}$ to $\sqrt{\frac{\dot{y}}{1-\left.\overline{\frac{y}{c}}\right|^{n+1}}}$ will be equal to that of $\frac{b}{c}$ to 1, or of *b* to *c*; this being the constant proportion of the fluxions when $x = \frac{by}{c}$; the fluents will be in the same proportion, provided $x = \frac{by}{c}$; wherefore the entire fluent of $\sqrt{\frac{\dot{x}}{b^{n+1}-x^{n+1}}}$ will be to the entire fluent of $\sqrt{\frac{\dot{y}}{c^{n+1}-y^{n+1}}}$ as $\frac{b}{b^{\frac{n+1}{2}}}$ to $\frac{c}{c^{\frac{n+1}{2}}}$; or as $b^{\frac{1-n}{2}}$ to $c^{\frac{1-n}{2}}$.

nution of the semiarc of vibration from B O to I O, will be $24^{h} \times \overline{\left.\frac{BO}{IO}\right|^{\frac{1-n}{n}} - 1}$.*

To apply this proposition, let a case be assumed; suppose a watch to be regulated to mean time when the semiarc of vibration is 135°, and let this semiarc be diminished 8°, so as to become 127°; let the ratio of the spring's elastic force deviate from that of the distances from the quiescent position by a small difference of $\frac{1}{1000}$ part, so that the spring's force shall be in the $\frac{999}{1000}$ power of the distances, instead of in the entire ratio of the said distances from the quiescent position. The alteration in the daily rate of the watch will be obtained from the preceding theorem by making the following substitutions. $BO = 135°$, $IO = 127°$, $n = \frac{999}{1000}$: the alteration of the daily rate $= 24^{h} \times \overline{\frac{135}{127}\Big|^{\frac{1}{2000}}} - 1 =$ $+ 2''.62$.

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* From this general expression it appears, that when $n = 1$, that is when the spring's elastic force varies in the precise ratio of the angular distances of the balance from the quiescent position, the alteration of the daily rate in consequence of a diminution of the arc of vibration is $= 0$; because in that case $\overline{\left.\frac{BO}{IO}\right|}^{\frac{1-n}{n}} = 1$, and $\overline{\left.\frac{BO}{IO}\right|}^{\frac{1-n}{n}} - 1 = 0$. When n is less than 1, or when the force varies in a less ratio than that of the distances from quiescence, the rate will be accelerated, because in that case $\overline{\left.\frac{BO}{IO}\right|}^{\frac{1-n}{n}}$ will be greater than 1; and $\overline{\left.\frac{BO}{IO}\right|}^{\frac{1-n}{n}} - 1$, will be a positive quantity: but when n is greater than 1, or when the force varies in a ratio greater than that of the distances from quiescence, the rate will be retarded, because in that case $\overline{\left.\frac{BO}{IO}\right|}^{\frac{1-n}{n}}$ will be less than 1, and $\overline{\left.\frac{BO}{IO}\right|}^{\frac{1-n}{n}} - 1$ becomes negative. The converse of these propositions is likewise derived from the general theorem.

Whenever therefore it is found, by observing the rate of a time-keeper, that a diminution of the arc of the balance's vibration causes an acceleration of the daily rate, it is necessary to conclude, that the elastic force of the spring in this case varies in a ratio less than that of the distances from the quiescent position. In like manner, when a diminution of the arc of vibration causes a retardation of the rate, it is certain that the spring's elastic force varies in a higher ratio than that of the distances from quiescence. It appears, indeed, from some experiments, that the weights which counterpoise a spiral spring's elastic force, when wound to different distances from the quiescent position, are in the ratio of those distances; but it is shewn from this proposition, and the annexed table, that the differences between the weights, by which the ratio of the distances, and a ratio a little less is indicated, although far too small to be discoverable by experiment, are yet sufficient to create a material alteration of the daily rate.

It here appears, that a very minute alteration in the law of the force's variation, amounting to no more than $\frac{1}{1000}$ part of the entire ratio of the distances, causes an acceleration in the daily rate of more than $2\frac{1}{2}''$, when the diminution of the semiarc of vibration is 8°. It may therefore be of some use to inquire, what are the differences of the weights to be observed in experiments from which the law of the spring's elastic force is derived; first, supposing that law to be the precise ratio of the distances from the quiescent position; and secondly, supposing the law of the force to deviate from that ratio by a small difference of $\frac{1}{1000}$, so as to become the $\frac{999}{1000}$ power of the distances from the quiescent position; from the result a judgment may be formed how far experiments may be relied on for ascertaining the precise law according to which the elastic force of a spring varies.

Values of x°.	Values of $P = 9\text{gr.} \times \frac{x^\circ}{90^\circ}$	Values of $P = 9\text{gr.} \times \left(\frac{x^\circ}{90^\circ}\right)^{\frac{999}{1000}}$
Angular distances from the quiescent position when the spring's elastic force is counterpoised by the weights in the second and third columns. (Fig. 2).	Weights P, expressed in grains, which counterpoise the spring's elastic force when wound to the several distances from the quiescent position in the first column, if the force varies in the precise ratio of the angular distances from the quiescent point.	Weights P, expressed in grains, which counterpoise the spring's elastic force when wound to the several distances from quiescence in the first column, if the force varies in the $\frac{999}{1000}$ power of the distances from the quiescent point.[*]
	Grains.	Grains.
10°	1	1.002199
20°	2	2.003010
30°	3	3.003298
40°	4	4.003245
50°	5	5.002940
60°	6	6.002433
70°	7	7.001759
80°	8	8.000942
90°	9	9.000000

The differences of weights expressed in the second and third columns of this table are evidently too small to admit of being observed experimentally, and yet their effect on the daily rate of a time-keeper amounts to a quantity far from insensible. This effect on the rate might probably be augmented to twenty or thirty seconds daily, and yet the corresponding differences of weights arising from the deviation of the spring's force from the law of isochronism might be too minute to become sensible by any statical counterpoise of the spring's forces; and it would be still less possible to measure the said differences of weights with the exactness required for the determination of the law observed by the spring's forces. Experiments of this kind should not therefore be absolutely relied on for ascertaining practically the isochronal property of spiral springs, although this property must be allowed to exist in theory, whenever the forces of elasticity at the several angular distances from the quiescent position are in the precise ratio of those distances. The isochronal law of variation, here mentioned, may be conveniently assumed in theoretical investigations, and proper corrections or equations, when necessary, may be applied to compensate for the deviation from this law, which may subsist in any particular spiral spring, whenever it can be satisfactorily ascertained, or reduced within known limits, by such mode of inference as the nature of the case may admit of. This assumption will appear the less exceptionable from considering, that the elastic forces of spiral springs which are not isochronal deviate from the law of variation in question, in some cases by exceeding, and in others, by falling short of it; and no other law is suggested either by theory or experiment, which more generally corresponds with the action of balance springs.

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[*] It may be here observed, that the differences of the weights in the second and third columns of the table, first increase and afterwards decrease; their difference is the greatest when the quantity $9 \times \left(\frac{x}{90}\right)^{\frac{999}{1000}} - \frac{9x}{90}$ is a maximum; or when $x = 90^\circ \times \left(\frac{999}{1000}\right)^{1000}$, that is when $x = 33^\circ\, 5'\, 33''$.

The vibration of a balance impelled by a single spiral spring only, has been the subject of the preceding investigations; but cases occur in which two or more springs are employed in giving vibratory motion to the balances of watches. Not to mention preceding instances, Mr. MUDGE, an eminent watch-maker of the present times, has invented a method of combining the action of spiral springs, to impel the balance in each semiarc of vibration, on a principle not more remarkable for the novelty than it is for the ingenuity of the contrivance. The consideration of this additional case will therefore not be thought foreign to the present subject, especially as it may contribute to elucidate some circumstances respecting the effect of springs on the vibrations of balances, which at the first view are not at all obvious.

Let two spiral springs be applied to act on a watch balance in the same direction; if the two springs in unwinding themselves, by turning the balance come to the same point of quiescence, or, in other words, if the accelerative forces of both springs cease at the middle point of the vibration, whatever be the relative strength of the two springs, they will act on the balance precisely in the same manner as if one spring only had been applied, of equal strength with both (the springs being here supposed similar, in respect of the law of the elastic forces and tensions). But when the points of quiescence of the two springs do not coincide, that is, when one spring continues to accelerate the balance in its vibration, after the acceleration of the other spring has ceased, the time of a semivibration must be obtained from a separate investigation.

Let the circumference of a balance (fig. 4.) be impelled by the action of a spiral spring through the semiarc of vibration B O, the forces of this spring being always in proportion to the angular distances from the point of quiescence O; let a secondary or auxiliary spring also act on the balance from the extremity of the semiarc B as far as the point Q, at which point all acceleration of the auxiliary spring ceases, the forces of the auxiliary spring varying as the angular distances from the point of quiescence Q. Suppose that the accelerative force of the principal or balance spring on the circumference of the balance at the distance from quiescence O D is $=f$, take Q $d =$ O D, and let the accelerative force of the balance spring on the circumference of the balance be to that of the auxiliary spring, when both springs are wound to the same angle O C D = Q C *d*, in the proportion of 1 to *n*; then the accelerative force of the auxiliary spring at the angular distance from quiescence Q *d* will $= nf$; let B O $= b$, B Q $= c$, Q O $= d = b - c$; also let O D or Q *d* $= a$.

Suppose the balance to have described the arc B H by the joint action of both the springs, and let the arc B H be represented by x. Then, because the accelerative force of the principal or balance spring at the angular distance from quiescence O D $= a$ is f, the accelerative force of the same spring at the distance O H is $= \frac{f \times \overline{b - x}}{a}$; and since the force of the auxiliary spring at the angular distance from quiescence Q $d = a$ is nf, the accelerative force of this spring at the angular distance from quiescence Q H will be $= \frac{nf \times \overline{c - x}}{a}$; wherefore the joint force of both springs to accelerate the circumference when at the distance O H from the point of quiescence O, that is, when the balance has described the arc B H, will be $= \frac{f}{a} \times \overline{b - x + cn - nx}$. Let u be the space through which a body falls freely from rest by the acceleration of gravity, to acquire the velocity of the circumference when it has described the arc B H; this will give the following equation:

$\dot{u} = \frac{f}{a} \times \overline{b\dot{x} - x\dot{x} + cn\dot{x} - nx\dot{x}}$; and

$u = \frac{f}{2a} \times \overline{2bx - x^2 + 2cnx - nx^2}$; and if $l = 193$ inches, the velocity of the circumference when the arc B H has been described, is $= \sqrt{\frac{2lf}{a}} \times \sqrt{2bx - x^2 + 2cnx - nx^2}$. Let t represent the time in which the balance describes the arc B H; then $\dot{t} = \sqrt{\frac{a}{2lf}} \times \frac{\dot{x}}{\sqrt{2bx - x^2 + 2cnx - nx^2}}$ and the fluent or $t = \sqrt{\frac{2a}{lf \times \overline{n + 1}}} \times$ into a circular arc of which the sine is

$\sqrt{\frac{x \times \overline{n+1}}{2b+2nc}}$ to radius $= 1$; which is the time of describing the arc B H. When $x = c$, the time of describing the arc B Q, is $\sqrt{\frac{2a}{lf \times \overline{n+1}}} \times$ into a circular arc of which the sine $=$ $\sqrt{\frac{c \times \overline{n+1}}{2b+2nc}}$.

The time of describing the remaining part of the semiarc Q O, (fig. 4.) is next to be determined. In the cases to which this investigation is applied, the auxiliary spring ceases entirely to act on the balance after it has described the arc B Q. This being stated, while the balance describes the remaining arc of the semivibration Q O, it will be impelled by the balance spring only. To ascertain the time of describing the arc Q O, it is first to be observed, that when the circumference has described the arc B Q, it will have acquired a velocity equal to that of a body which has fallen from rest by the acceleration of gravity through a space* $= \frac{f}{2a} \times \overline{2bc - c^2 + nc^2}$.

Suppose the balance to have proceeded through the arc Q R, (fig. 4.) and let O R $= x$; the force by which the circumference is accelerated at R $= \frac{fx}{a}$; and if u is the space through which a body falls freely from rest by the acceleration of gravity to acquire the velocity of the circumference in the point R, $\dot{u} = \frac{-f\dot{x}x}{a}$; taking the fluents so that u may become $=$ $\frac{f}{2a} \times \overline{2bc - c^2 + nc^2}$ when $x = d$, $u = \frac{f}{2a} \times \overline{2bc - c^2 + nc^2 + d^2 - x^2}$; or because $d^2 = b^2 - 2bc + c^2$; $u = \frac{f}{2a} \times \overline{b^2 + nc^2}$

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* When the circumference has described the arc B H $= x$, it will have acquired a velocity equal to that of a body which has fallen freely from rest by the acceleration of gravity through a space $= \frac{f}{2a} \times \overline{2bx - x^2 + 2cnx - nx^2}$, as appears from the investigation in page 138; and when $x = c$, this space becomes $= \frac{f}{2a} \times \overline{2bc - c^2 + 2c^2n - nc^2} = \frac{f}{2a} \times \overline{2bc - c^2 + nc^2}$.

$\overline{-x^2}$; and the velocity of the circumference at R (fig. 4.) $= \sqrt{\frac{2lf}{a}} \times \sqrt{b^2+nc^2-x^2}$: let t be the time of describing QR; then $\dot{t} = \sqrt{\frac{a}{2lf}} \times \frac{-\dot{x}}{\sqrt{b^2+nc^2-x^2}}$, and $t = \sqrt{\frac{a}{2lf}} \times$ into a circular arc of which the cosine is $\frac{x}{\sqrt{b^2+nc^2}}$ to radius = 1, which should = o when $x = d$; wherefore t or the time of describing QR $= \sqrt{\frac{a}{2lf}} \times$ into a circular arc of which the cosine is $\frac{x}{\sqrt{b^2+nc^2}} - \sqrt{\frac{a}{2lf}} \times$ into a circular arc of which the cosine is $\frac{d}{\sqrt{b^2+nc^2}}$; and when $x = o$, that is when the entire arc Q O has been described, the time $t = \sqrt{\frac{a}{2lf}} \times$ into a circular arc of which the sine is $\frac{d}{\sqrt{b^2+nc^2}}$.

The result of this investigation is, that the time in which the balance describes the semiarc B O, by the joint action of both springs through the arc B Q, and by the action of the balance spring only through the arc Q O,

is $= \sqrt{\frac{2a}{lf \times \overline{n+1}}} \times$ an arc, of which the sine is $\sqrt{\frac{c \times \overline{n+1}}{2b+2nc}}$

$+ \sqrt{\frac{a}{2lf}} \times$ an arc, of which the sine is $\frac{d}{\sqrt{b^2+nc^2}}$

(to radius = 1) expressed in parts of a second.*

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* When $n = o$, this expression becomes $\sqrt{\frac{a}{2lf}} \times \overline{\text{twice an arc, of which the sine is } \sqrt{\frac{c}{2b}}} + $ an arc, of which the sine is $\frac{d}{b}$: but since $c = b - d$, the two arcs here mentioned will be exactly $= 90° = \frac{p}{2}$; and the time of a semivibration $= \sqrt{\frac{a}{2lf}} \times \frac{p}{2} = \sqrt{\frac{ap^2}{8lf}}$, agreeing with the solution in page 126. Suppose $d = o$; since in this case $b = c$, the time of a semivibration becomes $\sqrt{\frac{2a}{lf \times \overline{n+1}}} \times$ an arc, of which the sine is $\sqrt{\frac{b}{2b}}$ which arc is $= 45°$, or $\frac{p}{4}$; wherefore the time of a semivibration in this case $= \sqrt{\frac{ap^2}{8lf \times \overline{n+1}}}$, which is the true value, according to the solution in page 126. See also page 146.

This solution is confined to that case in which the point of quiescence Q (fig. 4.) of the auxiliary spring is situated in the first semiarc of the vibration, that is, between B and O. Another case still remains to be considered, which is, when the point of quiescence Q of the auxiliary spring deviates from O by the given angular distance OQ, but is situated in the latter semiarc of the vibration (fig. 5.), between O and E, instead of between O and B, as in the former solution. According to this condition, making $BQ = c$, $BO = b$, and the other notation remaining as before, it appears from an investigation no ways differing from the preceding, that the time in which the balance describes the semiarc BO will be

$t = \sqrt{\frac{2a}{lf \times \overline{n+1}}}$ × into a circular arc, of which the sine is $\sqrt{\frac{b \times \overline{n+1}}{2b + 2nc}}$ expressed in parts of a second.

This result expresses the time in which the balance describes the semiarc BO, (fig. 5.) by the accelerative force of two springs, namely, the balance spring, of which the point of quiescence is O, and an auxiliary spring, of which the point of quiescence is Q. On considering this case more fully, when applied to the actual vibrations of a balance, it will appear evident, that the action of a third spring on the balance while it is describing the semiarc BO, must be taken into the calculation, in addition to the two springs already mentioned, in order to obtain a solution entirely correspondent with the circumstances of the case, when the points of quiescence of the auxiliary springs are situated in the latter semiarcs of the vibrations.

To state this more clearly, it is to be observed, that when the points of quiescence of the balance and auxiliary springs are coincident; the auxiliary spring commencing its action from the extremity B of the arc BO, (fig. 6.) continues to accelerate the balance till it arrives at the quiescent position O, at which point the action of the auxiliary spring entirely ceases; on this account it is plain that another auxiliary spring, equal and similar to the former, having also the point of quiescence coincident with O, must be applied to act by retardation on the balance while it describes the arc OE, in order that the times of describing the arcs BO and OE, as well as these arcs themselves, may be equal. According, therefore, to this disposition of the auxiliary springs, the balance will describe each semiarc of its vibrations precisely in the same manner as if it was impelled by one spiral spring only, the strength of which is equal to that of the balance and either auxiliary spring, when wound to the same tension. Suppose the balance to vibrate from B to E, and, for the sake of distinction, let the auxiliary spring which accelerates the balance from the extremity of the arc B, in the direction BO, be called u; and let the other auxiliary * spring which retards the balance in the semiarc OE be denoted by v; consequently, when the balance vibrates in the contrary direction from E to B, the auxiliary spring v will accelerate the balance from E to O, and the other auxiliary spring u will retard it from O to B. In respect, therefore, to the spring u, BO is the first semiarc, and OE is the latter semiarc of vibration; and on a similar principle in respect to the spring v, EO is the first semiarc, and OB is the latter semiarc of vibration.

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* The circular arcs which are drawn interior to the circumference of the balance in the figures 4, 5, and 6, are intended to represent those portions of the balance's vibration in which the auxiliary springs respectively act.

In the preceding pages, the time has been investigated in which a balance vibrates in the semiarc B O, (fig. 4.) when the point of quiescence Q, of the auxiliary spring *u*, is situated between B and O, that is in the first semiarc of vibration. In this case, in order that the time of describing the latter semiarc O E, by retardation, may be equal to that of describing the first semiarc B O by accelerated motion, the quiescent point of the auxiliary spring *v* must be placed at N, between O and E, so that E N may be equal to B Q; according to this disposition of the auxiliary springs, the point of quiescence Q of the auxiliary spring *u* will be situated in the first semiarc of vibration, considering the balance as vibrating from B to E; and the point of the quiescence N of the auxiliary spring *v*, will be in the first semiarc of vibration, considering the balance as vibrating from E to B.

The third case which remains to be more fully investigated is, when the points of quiescence of the auxiliary springs are situated in the latter semiarcs of the respective vibrations; that is, when the quiescent point Q of the auxiliary spring *u* is situated in the latter arc O E, (fig. 5) while the balance vibrates from B to E, and the point of quiescence N of the auxiliary spring *v* is situated in the latter arc O B, while the balance vibrates from E to B. In this case it is evident, that while the balance vibrates from B to O, it describes the arc B N by the acceleration of two springs; namely, the balance spring, of which the point of quiescence is O, and the auxiliary spring *u*, of which the point of quiescence is Q. And the balance describes the arc N O by the combined action of three springs; namely, by that of the balance spring, of which the point of quiescence is O, that of the auxiliary spring *u*, of which the point of quiescence is Q, both of which accelerate the balance, and that of the auxiliary spring *v*, of which the point of quiescence is N, acting by retarding the balance.

The time in which the balance describes the semiarc BO, by the combined action of these three springs, will be obtained by the following investigation.

Resuming the former notation, let DO (fig. 5.) $= a = Qd = Ne$, $BO = b$, $BQ = c$, $OQ = ON = d = c - b$. The accelerative force of the balance spring on the circumference of the balance at the tension $OD = f$; the accelerative force of the auxiliary springs at the same tension Qd or $Ne = nf$. From page 138 and 139, it appears that the time in which the balance describes the arc BN by the joint action of the balance spring and auxiliary spring $u = \sqrt{\frac{2a}{lf \times \overline{n+1}}} \times$ a circular arc of which the sine $= \sqrt{\frac{\overline{b-d} \times \overline{n+1}}{2b + 2nc}}$; and the velocity acquired by the circumference at N is equal to that of a body which has fallen freely from rest by the acceleration of gravity through a space* $= \frac{f}{2a} \times \overline{b^2 - d^2 + nb^2 - 3nd^2 + 2nbd}$. To find the time of describing the arc NO, suppose the balance to have proceeded in its vibration from N to R, (fig. 5.) and make $NR = x$.

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* In the investigation, page 138, it is shewn, that when the balance has described the arc $BH = x$, the space through which a body must fall freely from rest to acquire the velocity of the circumference at the point H, or $u = \frac{f}{2a} \times \overline{2bx - x^2 + 2cnx - nx^2}$, the expression being the same, whether the point Q is on one side of O or on the other, provided $BQ = c$. In the present case, $c = b + d$, and when the circumference has described the arc BN, $x = b - d$; wherefore if $b + d$ is substituted for c, and $b - d$ for x in the equation, $u = \frac{f}{2a} \times \overline{2bx - x^2 + 2cnx - nx^2}$, the result will be $u = \frac{f}{2a} \times \overline{b^2 - d^2 + nb^2 - 3nd^2 + 2nbd}$.

The accelerative force of the balance spring on the circumference of the balance at the point R is $+\frac{f}{a}\times\overline{d-x}$

The accelerative force of the auxiliary spring u, at the tension or distance from quiescence $Qd=a$ being nf, the force of the same spring at the tension or distance $QR=2d-x$, is - - - - - - $+\frac{nf}{a}\times\overline{2d-x}$

The force of the auxiliary spring v, at the tension $Ne=a$ being nf, at the tension NR, the force of this spring acting by retardation will be - - - - - $-\frac{nf}{a}\times x$.

Sum of the forces acting on the circumference of the balance, when it has described the arc $NR=$ - - $\frac{f}{a}\times\overline{d-x+2dn-2nx}$.

Let u be the space through which a body falls freely from rest by the acceleration of gravity, to acquire the velocity of the circumference at R; the principles of acceleration give this equation; $\dot{u}=\frac{f}{a}\times\overline{d\dot{x}-x\dot{x}+2dn\dot{x}-2nx\dot{x}}$; and taking the fluents so that when $x=0$, u may be $=\frac{f}{2a}\times\overline{b^2-d^2+nb^2-3nd^2+2nbd}$; $u=\frac{f}{2a}\times\overline{b^2-d^2+nb^2-3nd^2+2nbd+2\overline{d+4nd}\times x-\overline{1+2n}\times x^2}$; if t is put to represent the time of describing the arc NR, $t=\sqrt{\frac{a}{2lf}}\times\frac{\dot{x}}{\sqrt{b^2-d^2+nb^2-3nd^2+2nbd+\overline{2d+4nd}\times x-\overline{1+2n}\times x^2}}$, and taking the fluents so that when $x=0$, $t=0$, and making $x=d$, the time of describing the arc $NO=\sqrt{\frac{a}{2lf\times\overline{1+2n}}}\times$ a circular arc of which the sine $=\sqrt{\frac{d^2\ \overline{1+2n}}{b^2+n\times\overline{b+d}^2-2nd^2}}$, or because

$b + d = c$, $t = \sqrt{\frac{a}{2lf \times \overline{1 + 2n}}}$ × a circular arc of which the sine $= \sqrt{\frac{d^2 \times \overline{1 + 2n}}{b^2 + nc^2 - 2nd^2}}$.

The result is, that when the points of quiescence of the auxiliary springs are situated in the latter semiarcs of the respective vibrations, the time in which the balance describes the semiarc B O will be $= \sqrt{\frac{2a}{lf \times \overline{n + 1}}}$ × a circular arc of which the sine is $\sqrt{\frac{\overline{b - d} \times \overline{n + 1}}{2b + 2nc}} + \sqrt{\frac{a}{2lf \times \overline{1 + 2n}}}$ × a circular arc of which the sine is $\sqrt{\frac{d^2 \times \overline{1 + 2n}}{b^2 + nc^2 - 2nd^2}}$.

The balance of Mr. MUDGE'S time-keeper describes the semiarc B O, by the joint action of two springs, *i. e.* the balance* spring, and an auxiliary spring; each spring is wound through the same arc B O, and comes to the same point of quiescence O (fig. 6.); consequently the action of the two springs is the same with that of a single spring of equal strength with both. In this case, the time of a semivibration through the arc B O, will be obtained from a former solution; for referring to page 126, and making O D $= a$, and the force of the balance spring and auxiliary spring at the distance from quiescence O D, $= f + nf = F$; the time of a semivibration is

$$= \sqrt{\frac{ap^2}{8lf \times \overline{n + 1}}} = \sqrt{\frac{ap^2}{8lF}}.$$

But if the points of quiescence of the balance and auxiliary springs instead of coinciding, according to the principle of Mr. MUDGE'S construction, should deviate from this adjustment by a small arc O Q = O N (fig. 4 and 5.); to what extent the daily rate of the time-keeper may be affected by this alteration in the position of the quiescent points, remains unknown, unless it be investigated by assuming the deviation of the points of quiescence, as one of the conditions on which the time of vibration depends. This condition is included in the preceding investigations, which may be now applied to the solution of some cases, which are suggested by considering the construction of Mr. MUDGE's time-keeper.

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* A double spiral spring is applied in the balance of Mr. MUDGE'S time-keeper, but as these two springs act as one spring, they are here considered as such.

Any minute or particular description of this ingenious invention would be foreign to the subject of theoretic investigation; an outline only of the construction on which the action of the several springs depends will be necessary, to render the application of the preceding theorems sufficiently intelligible.

ONEBQ (Tab. XV. fig. 8.) is the circumference of the balance, vibrating by the action of a spiral spring on an axis C A D H, passing through the centre C; the axis is discontinued from A to D to make room for the other parts of the work. C A and D H are connected by means of a branch or crank A X Y D, which is fixed to the axis C A D H, and always vibrates with the balance on the said axis.*

L M, Z W, are two rods affixed to the crank at the points L and Z parallel to X Y; which rods also vibrate with the balance. *c*, *d*, *e*, *f*, *g*, *r*, *s*, are fixed parts of the machine. T R is an axis in the same right line with C A D H carrying an arm G O at right angles to it † (or nearly so), and a small auxiliary spring *u*, which is wound up whenever the arm G O is turned round the axis T R in the direction of the arc O *b*; *p* is a curved pallet fixed to the axis T R, which receives the tooth of the balance wheel near the axis; the tooth proceeding along the curved surface by the force of the main spring, turns the axis and the annexed arm G O in the direction of the arc O *b*, and at the same time winds up the auxiliary spring *u*. A small projection at the extremity of the curved surface of the pallet *p* prevents further progress of the tooth, when the arm O G has been turned through an arc O *b*[*] of about 27°; consequently the spring *u* has then been wound through the same arc or angle O G *b* = 27°.

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* The additional weight affixed to the balance at *t* counterpoises the weight of the crank or branch A X Y D, so as to bring the centre of gravity of the whole into the axis of motion.

† In the machine G O and I O are not exactly at right angles to T R and F S; but are so represented in the figure, in order to make the different positions of the arms G O and I O the more distinct.

F S is another axis in the same right line with C A D H, exactly similar to that which has been described. This axis F S carries with it the arm I O; the auxiliary spring *v* is annexed to this axis, and is wound up when the axis is turned in the direction of the arc O *k*: *q* is a pallet similar to the former pallet *p*, and is placed so as to receive the tooth of the balance wheel, which by its action on the pallet winds up the spring *v*, and carries the arm O I through an angle O I *k*, = 27°, further motion being prevented by a small projection at the extremity of the pallet *q*, similar to that which has been already mentioned. *l m* represents the balance wheel, the upper tooth of which acts on the pallet *p*, and the lower tooth on the pallet *q*, alternately winding up the auxiliary springs *u* and *v*, in the manner described in the subsequent page: the axis of the balance wheel *n o* is parallel to the line C O or G O.

The several arcs expressed on the circumference of the balance, *i. e.* O Q, O *b*, O N, O *k*, are equal to the respective arcs denoted by the same letters in the circumference of the circles described by the extremities of the arms G O and I O.

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[*] *Vide infra* the note in page 150.

Suppose that when the balance is quiescent, the main spring being unwound, the branch or crank A X Y D is in the position represented in fig. 8, A X being parallel to C O; if the quiescent points of the auxiliary springs coincide with that of the balance spring, the arm G O will just touch the rod L M; and in like manner the arm I O will just touch the rod W Z; the two arms G O and I O, in this position, are parallel to the line C O. This position of the balance and auxiliary springs remains as long as the main spring of the machine continues unwound; but whenever the action of the main spring sets the balance wheel in motion, a tooth thereof meeting with one or other of the pallets *p* or *q*, will wind up one of the auxiliary springs; suppose it should be the auxiliary spring *u*: the arm G O being carried into the position G *b*, by the force of the balance wheel acting on the pallet *p*, remains in that position as long as the tooth of the balance wheel continues locked by the projection at the extremity of the pallet *p*: and the balance itself not being at all affected by the motion of the arm G O, nor by the winding up of the spring *u*, remains in its quiescent position; consequently no vibration can take place except by the assistance of some external force to set the machine in motion. Suppose an impulse to be given to the balance sufficient to carry it through the semiarc O B, which is about* 135°, according to Mr. MUDGE's construction.

The balance during this motion carries with it the crank A X Y D, and the affixed rods L M, Z W. When the balance has described an angle of about* 27° = to the angle O C *b* or O G *b*, the rod L M meets with the arm G *b*, and by turning the axis T R, and the pallet *p*, in the direction of the arc O *b*, releases the tooth of the balance wheel from the projection at the extremity of the pallet *p*; the balance wheel immediately revolves, and the lower tooth meeting with the pallet *q* winds up the auxiliary spring *v*, and carries the arm I O with a circular motion through the angle O I *k* about 27°; in which position the arm I O remains as long as the tooth of the balance wheel is locked by the pallet *q*. While the spring *v* is winding up through the arc O *k*, the balance describes the remaining part of the semiarc *b* B, and during this motion the rod L M carries round the arm G *b*, causing it to describe an angle *b* C B, or *b* G B[*], which is measured by the arc *b* B = [†]108°: when the balance has arrived at the extremity of the semiarc O B = 135°, the auxiliary spring *u* will have been wound up through the same angle = 135°, *i. e.* 27°, by the force of the main spring acting on the pallet *p*, and 108° by the balance itself carrying along with it the arm G O or G *b*, while it describes the arc *b* B. The balance therefore returns through the arc B O by the joint action of the balance spring, and the auxiliary spring *u*; the acceleration of both springs ceasing the instant the balance arrives at the quiescent point O; when the balance has proceeded in its vibration about 27° beyond the point O to the position C *k*, the rod Z W meets with the arm I *k*, and by carrying it forward releases the tooth of the balance wheel from the pallet *q*; the balance wheel accordingly revolves, and the upper tooth meeting with the pallet *p*, winds up the auxiliary spring *u* as before. The balance with the crank proceeding to describe the remaining semiarc *k* E, winds up the spring *v* through the further angle *k* C E = 108°; the balance returns through the semiarc E O, by the joint action of the balance spring and the auxiliary spring *v*, both of which cease to accelerate the balance the instant it has arrived at O.

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* *Vide infra* the note in page 150.

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[*] The angle *b* G B is not expressed in the figure for want of room, but is easily imagined, being precisely equal to the angle *b* C B represented on the balance.

[†] This magnitude of the arc *b* B has been inferred from the following circumstances communicated by Mr. MUDGE. 1st. "The whole arc of vibration is about $\frac{3}{4}$ of "a circle, or 270°, consequently the semiarc of vibration is 135°. 2dly. The ac"tion of the balance by carrying round the arm O G from *b* to B winds up the auxi"liary spring through an angle about four times greater than the angle O C *b* through "which it is wound up by the action of the balance wheel on the pallet." Wherefore if O B, or 135°, is divided into five parts, one of them will be O *b* = 27°, and the other four parts, or *b* B = 108°.

It is remarkable, according to this construction, that no force or impulse whatever is communicated to the balance from the main spring, and yet the vibrations are continued of their due length: on further consideration it appears that the maintaining power of the machine, instead of communicating any force or impulse, acts by removing a part of the force which retards the balance while it is describing the latter semiarc of each vibration.

In the preceding account it has been shewn, that the balance describes the semiarc from B to O by the joint action of the two springs; now for a moment let it be supposed that the balance vibrated through the entire arc B O E (fig. 7 and 8.) by the joint action of the two springs, in the same manner as if one balance spring only was applied of the same strength with both; in this case, the balance commencing its vibration at the extremity of the arc B, after having passed the semiarc B O with an accelerated motion, would describe an equal arc O E on the other side of O, by retarded motion, provided it was not obstructed by friction or other irregular resistances; but such resistances taking place will cause the latter semiarc, which is described by retarded motion, to fall short of the arc O E by some small difference E S. There are two modes by which this latter semiarc may be restored to its due length O E; either by communicating an impulse to the balance from the main spring, or by removing a part of the force which retards the ascent of the balance while it describes the latter semiarc O E. Mr. Mudge has discovered and applied to his time-keeper the latter mode of supplying the power which is required to continue the balance in motion. During the progress of the balance through the semiarc B O, it is accelerated by the joint action of the balance spring and auxiliary spring *u*; but while it describes the latter semiarc O E (fig. 7 and 8.), it is retarded by the joint actions of the balance spring and auxiliary spring, only while it describes a part of the semiarc from *k* to E, the retardation of the auxiliary spring being removed while it describes the first 27° of this semiarc from O to *k*.

It is evident, according to Mr. Mudge's construction, that the diminution of retardation, which is equivalent to the supply of power to the balance, must always be of the same magnitude, so far as regards the influence of the main spring, provided it has sufficient force to wind up the auxiliary springs through the constant angle O G *b* = O C *k* (fig 8.); and the effects of heat and cold on the auxiliary springs are necessarily included in the compensation which is applied for heat and cold to the balance spring. The maintaining power therefore, by which the motion of the balance is continued, must be always uniformly the same; this is an object usually held to be of material consequence in the construction of watches, and though often attempted by ingenious persons, has probably been accomplished in its full extent for the first time by Mr. Mudge.

This construction possesses the further advantage of having the balance perfectly detached from the wheel work of the machine; the only communication* between the balance and the balance wheel is that which subsists while the pallet is disengaged from the tooth; an instant of time in a practical sense almost evanescent: it should also be remarked, that the pressure of the tooth against the projection of the pallet is diminished before the tooth is released, by the counteraction of the other pallet on the opposite tooth of the balance wheel; which action takes place just before the tooth of the balance wheel first mentioned is released from the pallet: by this diminution of pressure, the tooth is the more easily detached from the projection of the pallet. These circumstances concur in giving to the machine facility and lightness of motion.

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* The exact times in which the balance describes any portions of the arc of vibration may be readily obtained by having recourse to the theorem investigated in page

The preceding investigations are in the next place to be ap-

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126. If we can ascertain the arc described by the balance while the tooth of the balance wheel is released from the projection of the pallet, the portion of time in which the balance is connected with the wheel-work of the machine will be known. By an experiment made on a very exact model of Mr. MUDGE's construction, it appeared that the balance described an arc of about 8°, while the tooth was released from the pallet. And since the balance describes 108° of its semivibration before the pallet begins to be moved, it will have described 116° before the tooth is released; we are therefore to find the times in which the balance describes severally the two arcs 108° and 116°: the difference of these times will be the time in which the balance wheel is released from the pallet. By the theorem referred to

	Parts of a second.
The time in which the balance describes an arc of 116° is -	0.09101
Time of describing an arc of 108° is - - -	0.08718
The difference, or time during which the balance is connected with the other parts of the machine, - - - -	0.00383

which is about $\frac{1}{260}$ part of a second of mean time, the effects of friction not being here considered.

If therefore the time of a semivibration is supposed to be divided into 100 equal parts, or physical instants, during the first 91 of them the balance will describe 108° from the extremity of the arc of vibration, to the contact of the arm connected with the pallet. During the 4 succeeding instants the pallet will be released, and during the 5 instants following, the balance will describe the remaining arc of the semivibration. Thus it appears, that the balance moves perfectly free and unconnected with the wheelwork of the machine during 96 parts in 100 of the time of each vibration.

plied to ascertain what alteration of the daily rate is occasioned in consequence of any deviation from coincidence of the points of quiescence of the balance spring and auxiliary springs, (fig. 8, and fig. 4 and 5.) that may have taken place either through a casual derangement of their position, or from a purposed adjustment. This inquiry offers three cases, which are to be separately considered; the first is, when the points of quiescence of the auxiliary springs coincide with the point of quiescence of the balance spring. 2dly. When the point of quiescence Q of the auxiliary spring *u* is situated in the first semiarc of the vibration, or between B and O (fig. 4 and 8.). 3dly. When Q is situated in the latter semiarc of vibration between E and O (fig. 5.); the like cotemporary position is to be understood in respect of the other auxiliary spring.

The consideration of these cases requires that the accelerative[*] forces of the balance spring and auxiliary springs, when at a given tension, should be known. This point will not be difficult to ascertain, if the proportion of the spring's forces at the same tension is given. By information received from Mr. Mudge, it appears that the force of each auxiliary spring is about $\frac{1}{20}$ part of the force of the balance spring. Assuming this proportion, let the points of quiescence of the auxiliary springs be supposed to coincide with the point of quiescence of the balance spring, and let the radius of the balance be taken at one inch; the watch being adjusted to mean time, when the balance makes five vibrations in a second, if the force of gravity is $= 1$, the force which accelerates the circumference at an

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[*] The accelerative force being always proportional to the absolute forces of impulse (as measured or counterpoised by a weight) divided by the mass moved equivalent to the resistance of inertia, the latter quantity remaining the same in the case of a balance's vibration, the accelerative forces will be in the same ratio with the forces of impulse.

angular distance of 90° from the quiescent point will be = 1.00408926 (p. 129): this force, in the present instance, is compounded of the force of the balance spring, and that of the auxiliary spring, which are assumed to be in the proportion of 20 to 1; consequently the force of the balance spring to accelerate the circumference at the tension 90°, or $f =$ 0.9562754
and the force of the auxiliary spring at the same tension, or $\frac{1}{20}$ of $f =$ - - - .0478138

Joint force of both springs to accelerate the circumference at the tension, 90° or $f + \frac{1}{20} f =$ 1.0040892

With* this force the balance vibrates five times in one se-

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* From the investigation in page 128 it appears, that if F is the accelerative force on the circumference of the balance at the angular distance from quiescence c°, $p = 3.14159$, &c. $r =$ the radius of the balance expressed in inches, $l = 193$ inches; the time of a semivibration will be $= \sqrt{\frac{p^3 r c^\circ}{8 l F \times 180^\circ}}$.

In the present case $c^\circ = 90^\circ$, $r = 1$ inch $F = f + \frac{1}{20} f = 1.0040892$; wherefore the time of a semivibration $= \sqrt{\frac{p^3 r c^\circ}{8 l F \times 180^\circ}} = \frac{1}{10}$ part of a second precisely.

If the balance should vibrate by the force of the auxiliary springs only, the force of acceleration on the circumference at the distance 90° from quiescence is $\frac{1}{20} f = 0.0478138$; wherefore $\frac{1}{20} f$ being substituted for F in the quantity $\sqrt{\frac{p^3 r c^\circ}{8 l F \times 180^\circ}}$ the other values remaining the same as in the former case, the time of a semivibration will now be $\sqrt{\frac{p^3 r c^\circ}{8 l \times \frac{1}{20} f \times 180^\circ}} = \overset{\text{pts. of a sec.}}{.45825}$; and the time shewn by the watch in any portion of mean time t will $= \frac{t}{4.5825} = t \times .21822$. Thus if t should be taken = to one minute, or 60 seconds of time, the time shewn by the watch in 60 seconds will be $= 60 \times .21822 = 13$ seconds nearly.

This would be the result if the forces of the balance and auxiliary springs were precisely in the proportion of 20 to 1. Mr. Mudge mentioned this proportion by esticond when adjusted to mean time; the daily rate will therefore in this case be = 0.

Now let Q (fig. 4 and 8.), or the point of quiescence of the auxiliary spring deviate from O, the point of quiescence of the balance spring, by an arc O Q; suppose this arc O Q to be = 1°; and let the point Q be situated in the first semiarc of vibration between O and B. The time of describing the semiarc B O will be ascertained on these conditions, by referring to the investigation (page 140), and making the following substitutions:

$a = 1.5707963$ = an arc of 90° to radius = 1 *

$b = 2.3561945$ = an arc of 135°

$c = 2.3387412$ = an arc of 134°

$b - c = d = 0.0174533$ = an arc of 1°

$l = 193$

$f = 0.9562754$

$n = \frac{1}{20}$.

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mation only, not having any memorandum of experiments made to ascertain the exact proportion.

If therefore the actual rate of the watch should be observed when the balance vibrates by the action of the auxiliary springs only, it is probable that the time shewn by the watch in 60 seconds of mean time might differ somewhat from that which has been here stated.

Since these notes were written, I have been favoured by his Excellency Count Bruhl with an account of an observation made on the rate of Mr. Mudge's first timekeeper, when the balance vibrated by the action of the auxiliary springs only, the balance spring being removed. According to this observation, the watch shewed twelve minutes by the motion of the hands in one hour of mean time, which corresponds to an interval of 12 seconds of time, shewn by the watch in 60 seconds, or one minute of mean time. According to the calculation, 13 seconds of time are shewn by the watch in one minute. A nearer agreement between the theory and matter of fact could scarcely be expected in the circumstances of the experiment.

* In all the following calculations the radius is also = 1.

Then, $\sqrt{\frac{2a}{lf \times \overline{n+1}}}$ × into a circular arc, of which the sine is

parts of a second.

$\sqrt{\frac{c \times \overline{n+1}}{2b+2nc}} =$ - - - 0.09955070

$\sqrt{\frac{a}{2lf}}$ × into a circular arc, of which the sine

is $\frac{d}{\sqrt{b^2+nc^2}} =$ - - - 0.00047174

Time of a semivibration in the arc BO = - .10002244

The time shewn by the watch in $24^h = \frac{24^\circ}{1.0002244} = 23^h\ 59'\ 40''.60$, giving a daily rate of 19″.40 slow.

This variation of the daily rate is not to be considered as affecting the regularity of the watch, as it is either compensated by adjustments when the watch is regulated to mean time, or taken as the established rate. A more material point is next to be determined; admitting the deviation of O and Q (fig. 4. and 8.) to be OQ = 1°, the same as in the former case; suppose the semiarc of vibration to be diminished from 135° to 125°. If the points of quiescence O and Q were coincident, this diminution of the arc of vibration would cause no alteration (page 128) in the time of a semivibration, because the forces of acceleration of both springs would be as the angular distances from the same quiescent point O; but since these points are separated by an arc of 1°, a diminution of 10° in the semiarc of vibration, will cause a change in the daily rate, which will be obtained from the general theorem (page 140) by making the following substitutions:

$a = 1.5707963 =$ an arc of 90°

$b = 2.1816616 =$ an arc of 125°

$c = 2.1642083 =$ an arc of 124°

$b - c = d = 0.0174533 =$ an arc of 1°.

$$l = 193$$
$$f = 0.9562754$$
$$n = \tfrac{1}{20}.$$

$\sqrt{\frac{2a}{lf \times \overline{n+1}}}$ × into a circular arc, of which the sine is

parts of a second.

$\sqrt{\frac{c \times \overline{n+1}}{2b + 2nc}} =$ - - - 0.09951472

$\sqrt{\frac{a}{2lf}}$ × a circular arc, of which the sine is

$\frac{d}{\sqrt{b^2 + nc^2}} =$ - - - - 0.00050949

The time of a semivibration - .10002421

Time shewn by the watch in $24^h = \frac{24^h}{1.0002421} = 23^h\ 59'\ 39''.08$

giving a daily rate of - - - 20″.92 slow.

Daily rate, when the semiarc of vibration was 135° 19″.40 slow.

Retardation of the daily rate in consequence of the diminution of the semiarc of vibration from 135° to 125° - - - - 1″.52.

In these examples, the point of quiescence of the auxiliary spring Q is situated in the first semiarc of vibration between B and O; and in consequence of this position, the daily rate of the time-keeper is retarded by a separation of the points of quiescence, while the semiarc of vibration remains the same; *i. e.* 135°; secondly, the separation of the points of quiescence remaining unaltered, a diminution of the arc of vibration causes a further retardation of the rate. Opposite effects on the daily rate take place when the point of quiescence Q is situated in the latter semiarc of vibration, (fig. 5.) between O and E. The arc OQ remaining = 1° as before, this case produces an acceleration instead of a retardation of the rate.

Suppose the semiarc of vibration B O to be 135°
Separation of the points of quiescence = O Q 1°

Arc B Q in fig. 5, or B N in fig. 8. - 136°

The time of describing the semiarc B O will be determined by referring to the theorem (page 146).

making $a = 1.5707963$ = an arc of 90°
$b = 2.3561945$ = an arc of 135°
$c = 2.3736478$ = an arc of 136°
$d = 0.0174533$ = an arc of 1°
$l = 193.$
$f = 0.9562754.$
$n = \frac{1}{20}.$

The time of describing the semiarc B O $= \sqrt{\frac{2a}{lf \times \overline{n+1}}}$ × a circular arc of which the sine is $\sqrt{\frac{\overline{b-d} \times \overline{n+1}}{2b + 2nc}}$ = 0.09950617 Parts of a second.

$\sqrt{\frac{a}{2lf \times \overline{1+2n}}}$ × a circular arc of which the sine is $\sqrt{\frac{d^2 \times \overline{1+2n}}{b^2 + nc^2 - 2nd^2}}$. - - = 0.00047141

Time of a semivibration in the arc B O = 0.09997758

Time shewn by the watch in $24^h = \frac{24^h}{.9997758} = 24^h\ 0'\ 19''.38$, giving a daily rate of 19.″38 fast.

Every thing else remaining, let the semiarc of vibration be diminished from 135° to 125°, and make

$b = 2.1816616$ = an arc of 125°
$c = 2.1991149$ = an arc of 126°

The time of describing B O will now be $\sqrt{\frac{2a}{lf \times \overline{n+1}}}$ × a

Parts of a second.

circular arc of which the sine is $\sqrt{\frac{\overline{b-d}\times\overline{n+1}}{2b+2nc}}$ = 0.09946662

$\sqrt{\frac{a}{2lf\times\overline{1+2n}}}$ × a circular arc of which the sine is $\sqrt{\frac{d^2\times\overline{1+2n}}{b^2+nc^2-2nd^2}}$ - - = .00050912

Time of a semivibration in the arc B O = .09997574

Time shewn by the watch in $24^h = \frac{24^h}{.9997574} = 24^h\ 0'\ 20''.91$, giving a daily rate of - - 20″.91 fast.

Daily rate when the semiarc of vibration was 135° - - - 19″.38 fast.

Acceleration of the rate in consequence of the diminution of the semiarc of vibration from 135° to 125° = - - - - 1″.53

The theorem from which this latter result has been calculated is founded on a supposition, that the arc B N (fig. 5.) is described by the accelerative forces of the balance spring and the auxiliary spring *u*, and that the arc N O is described by the accelerative forces of the two springs just mentioned, combined with the retarding force of the other auxiliary spring *v*, of which the point of quiescence is situated at N (fig. 5.). It may possibly be thought, that this supposed action of the auxiliary spring which retards the balance while it describes the arc from N to O, cannot take place, because by the peculiar construction of Mr. Mudge's invention, when the balance vibrates from B towards E, the retarding force of the auxiliary spring is removed from acting on the balance while it describes the arc N *k* = 27° (fig. 5.), including the said arc NO; but it is to be considered, that this removal of the retarding force from acting on the balance while it describes the arc from N to *k*, is merely a mechanical expedient for supplying the power which is lost by friction, and as it is thus wholly employed and counteracted, it appears requisite, in making calculations of the times in which the balance vibrates, that the entire forces of the springs are to be taken into the calculation in the same manner as if friction did not exist, and no mechanical contrivance was necessary for compensating the loss of motion in consequence of it. But since, in point of fact, no retarding force acts on the balance while it describes the arc N O, according to the conditions under consideration, it may be satisfactory to calculate the time of a semivibration in the arc B O on this ground also, that is, on a supposition that the entire semiarc B O is described by the joint accelerative forces of the balance spring, and the auxiliary spring *u* only; it will appear that this slight variation of the conditions does not at all affect the conclusions deduced from the preceding calculations, and very little alters the results themselves.

The time of describing the semiarc BO, on these conditions, will be determined by referring to the theorem (page 141), and making the following notations.

$a = 1.5707963$ = an arc of 90° to radius $= 1$
$b = 2.3561945$ = an arc of 135°
$c = 2.3736478$ = an arc of 136°
$d = 0.0174533$ = an arc of 1°.
$l = 193$
$f = 0.9562754$
$n = \frac{1}{20}$.

The time of a semivibration is $= \sqrt{\frac{2a}{lf \times \overline{n+1}}} \times$ into a circular arc, of which the sine $= \sqrt{\frac{b \times \overline{n+1}}{2b+2nc}} = \overset{\text{pts. of a second.}}{.09997752}$.

Time shewn by the watch in $24^{h} = \frac{24}{\cdot 9997752} = 24^{h}\ 0'\ 19''.42$, giving a daily rate of 19″.42 fast.

Every thing else remaining, let the semiarc of vibration be diminished from 135° to 125°; and make

$b = 2.1816616 =$ an arc of 125° to radius 1.

$c = 2.1991149 =$ an arc of 126°.

The time of describing the semiarc BO will now be $=$

$\overline{\frac{2a}{lf \times \overline{n+1}}} \times$ a circular arc, of which the sine $= \sqrt{\frac{b \times \overline{n+1}}{2b+2nc}}$ $= 0.09997570$ parts of a second.

Time shewn by the watch in $24^{h} = \frac{24^{h}}{\cdot 9997570} = 24^{h}\ 0'\ 20''.96$,

giving a daily rate of - - - 20″.96 fast.

daily rate, when the semiarc of vibration was 135° = 19″.42 fast.

Acceleration of daily rate in consequence of the diminution of the semiarc of vibration from 135° to 125° - - - - 1″.54,

scarcely differing from the former determination in page 160.

By having recourse to the figure, we may distinctly perceive the three quiescent positions of the arms GO and IO, (fig. 8.) in respect to the rods LM, ZW, which correspond with the several conditions assumed for the calculation of the preceding examples. In the first place, the crank AXYD, and the arms GO, IO, being in their quiescent position, it is evident from the preceding observations, that when the arms GO, IO are adjusted so as just to touch the rods LM, ZW, the points of quiescence of the auxiliary springs *u* and *v* coincide with the point of quiescence of the balance spring O, (fig. 6.) and in this case, the balance making 5 vibrations in a second when adjusted to mean time, the daily rate is = 0 (p. 146).

Secondly, suppose the arms G O and I O to be so affixed to the axes T R, F S, (fig. 8 and 4.) that instead of just touching the rods L M and N W when quiescent, they are inclined* to that position at any angle O G Q = O I N: the situation of the pallets *p* and *q* not being altered: in this case the point of quiescence of the auxiliary spring *u*, will be separated from the point of quiescence of the balance spring by the angular distance OGQ = OCQ. In like manner the point of quiescence of the auxiliary spring *v* will deviate from the point of quiescence of the balance spring by the angular distance OIN = OCN. Suppose that the arc OQ = ON is = 1°: the point of quiescence Q of the auxiliary spring *u* is in the first semiarc of vibration, that is, between B and O, (fig. 4.) while the balance vibrates from B to E; and the point of quiescence of the auxiliary spring *v*, is in the first semiarc of vibration between E and O, while the balance vibrates from E to B; on these conditions, the semiarcs of vibration B O and OE being 135°, the daily rate of the time-keeper will be 19″.40 slower than mean time, (p. 157) and if the semiarcs BO, O E, should be diminished from 135° to 125°, the daily rate will be 20″.92 slow (p. 158); the retardation of the daily rate in consequence of the diminished semiarc of vibration being 1″.52.

To consider the remaining case; if the arms G O, I O, (fig. 8.) are adjusted so as to press equally by the force of the auxiliary springs against the rods L M, W Z, when quiescent, the equal and contrary pressures prevent any apparent effect on the position of the balance or crank A X Y D; if the rod LM should be removed (by turning the balance in its plane), suppose that the arm GO rests in a position GN, at a distance beyond O, which is measured by the arc ON = 1°; in this position the point of quiescence of the auxiliary spring *u* will be situated in the latter semiarc of vibration at N, between E and O, and by a similar adjustment of the quiescent position of the arm IO, (fig. 5 and 8.) the point of quiescence of the auxiliary spring *v* will be situated at Q in the latter semiarc of the vibration, between O and B; in consequence of this position of the points of quiescence, the daily rate of the watch will be accelerated 19″.38, (p. 159) while the semiarc of vibration continues 135°, and when the semiarc of vibration is diminished to 125°, the daily rate will be further accelerated; the rate being 20″.91 fast, (page 160) or 1″.53 faster than when the semiarc of vibration was 135°.

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* The force of the main spring not being supposed to act on the balance wheel, or pallets *p* and *q*.

From these several* results it may be concluded, that although the rate of going of Mr. Mudge's time-keeper depends materially on the quiescent position of the arms G O, I O, (fig. 8.), that is, on the position of the points of quiescence of the auxiliary springs, yet while that position remains unaltered, whatever it may be, the regularity of the time-keeper will not be affected in consequence of the said position, provided the semiarc of vibration continues the same; but when this arc is diminished, an acceleration or retardation of the daily rate will take place, according to the situation of the points of quiescence of the auxiliary springs referred to the semiarcs of vibration in which these springs respectively act. If the points of quiescence of the auxiliary springs should be situated in the first semiarcs of vibration, a diminution of these arcs will cause a retardation of the rate; but if the points of quiescence should be situated in the latter semiarcs of the respective vibrations, the daily rate will be accelerated.

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* It is scarcely necessary to observe, that although these results have been investigated from supposing the elastic forces of the spiral springs to be in the precise law of the tensions, or distances from quiescence; yet if the spring's forces should deviate somewhat from that law, the general conclusions deduced from the preceding calculations, respecting the acceleration or retardation of the rate arising from an alteration in the position of the points of quiescence, will still be true, although the degree in which these effects take place may not be exactly the same as when the spring's forces are in the precise law assumed in the investigations.

By means of Mr. Mudge's construction, we may apply the principles deduced from the preceding investigations to correct such alterations in the daily rate as may arise from a diminished or increased arc of vibration. For if it should be known, from any satisfactory mode of trial, that the properties* of the balance spring are such as cause the longer arcs of vibration to be described in less time than the shorter arcs; whenever the arc of vibration is diminished, the time-keeper will lose; and this error in the rate would be corrected by adjusting the points of quiescence of the auxiliary springs in the respective latter semiarcs of vibration at such a distance O N

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* Different opinions have been entertained respecting the times in which a balance, vibrating freely by the action of a spiral spring, describes the longer and shorter arcs of vibration. Mr. Harrison says, "large arcs are naturally performed in less time than "small ones."—Notes taken at the Discovery of Mr. Harrison's Time-keeper, p. vii. In this opinion he is followed by M. Berthoud; "J'ai appris par des experiences sûres, "que les grand arcs et les petits arcs d'un balancier ne sont pas isochrones, et qu'en "general, dans un balaucier libre, les grands arcs sont plus prompts que les petits." Mr. Ludlam, in his report addressed to the commissioners of the board of longitude, and intituled, a Short View of the Improvements made or attempted in Mr. Harrison's Watch, has the following remark: "The principle on which Mr. Harrison forms "the alteration of the third part (before described) is, that the longer vibrations of a "balance moved by the same spring are performed in less time. This is contrary to "the received opinion among philosophers and workmen."

These contradictory opinions may possibly have arisen from experiments, in which

= OQ (fig. 8 and 5.) from the point of quiescence of the balance spring, as corresponds with the error intended to be rectified.* In like manner, if the property of the balance spring should be such that a diminution of the arc of vibration causes an acceleration of the daily rate, this error will be corrected by placing the points of quiescence of the auxiliary springs in the first semiarcs of vibration, (fig. 4 and 8.) at their proper distances from the point of quiescence of the balance spring. It is, however, to be remarked, that if the balance spring should be of the latter kind here assumed, and the points of quiescence of the auxiliary springs, either by any casual derangement in their position, or by an adjustment of them for the purpose of experimental observation, should be placed in the latter semiarcs of vibration, the effect of this position would be an acceleration of the rate, whenever the semiarc of vibration is diminished: and this effect would be produced on a double account; first, from the assumed nature of the balance spring, which disposes it to describe the smaller arcs in less time

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all the circumstances capable of influencing the times of vibration in longer or shorter arcs, were either not noticed, or omitted to be properly allowed for; this will seem the more probable if it be allowed, that a balance spring may be adjusted in various ways so that either the longer or shorter arcs shall be performed in the least time; not only by altering the thickness and strength of the spring in different parts, but, if we subscribe to the opinion of M. BERTHOUD, in a spring uniform in every respect throughout, by altering the length and number of turns. He infers, that a certain length and number of turns may be given to an uniform spiral spring which will make it perfectly isochronal. This latter principle, however, does not appear to have been verified by any satisfactory experiments. According to the inferences deduced from the preceding investigations, three spiral springs, which are not isochronal when acting singly, may be so united by properly adjusting their points of quiescence, that their combined action shall cause the balance to perform its vibrations in any two arcs of unequal lengths in the same time.

* The position of the points of quiescence of the auxiliary springs is here understood to be altered, by affixing the arms GO, IK, differently on the axes TR, FS; the quiescent position of the pallets being no ways changed.

than the larger arcs of vibration: and secondly, from the position of the points of quiescence of the auxiliary springs. But it is evident from the preceding considerations, that although the balance spring should not be isochronal, yet the regularity of the time-keeper will not be at all affected, however the points of quiescence of the auxiliary springs may be situated in respect of the point of quiescence of the balance spring, as long as the semiarc of vibration continues unchanged; and if the semiarc of vibration should be liable to increase or diminution, Mr. MUDGE's construction affords an effectual remedy against this cause of variation in the rate, since the arms projecting from the auxiliary springs may be so adjusted, as to place their points of quiescence either in the first or latter semiarcs of vibration, according as the balance spring, when acting singly, causes the shorter or longer vibrations to be described in the least time.

On revising the preceding pages a few observations have occurred, which may be here inserted.

Note to page 122, *line* 13.—The elastic force of the spiral spring when at a given tension to turn the balance, is here assumed to be the same, whether the balance is at rest or in motion, being in both cases equal to the weight by which the spring's force at the given tension would be counterpoised.

Note to page 131.—It is not necessary to add constant quantities to the fluents of the fluxions $\frac{-\dot{x}}{\sqrt{b^{n+1}-x^{n+1}}}$, $\frac{-\dot{y}}{\sqrt{c^{n+1}-y^{n+1}}}$; because when the entire fluents are taken, they are precisely in the same proportion, whether the constant quantities (or corrections as they are sometimes termed) are added or omitted.

Addition to the note in page 140.—If the points of quiescence are in the first semiarc of vibration, and $c = 0$, the point B will coincide with Q (fig. 4.), from which point the vibration will commence; in this case the expression for the time of a semivibration will become $t = \sqrt{\frac{a}{2lf}} \times$ an arc of which the sine is $\frac{d}{d}$, or an arc of $90° = \frac{p}{2}$, or $t = \sqrt{\frac{ap^2}{8lf}}$; which agrees entirely with the solution in page 126; for in this case the auxiliary spring not acting on the balance while it describes the arc QON, the balance will vibrate by the force of the balance spring only; of which the force at the distance a or 90° is $= f$, and consequently by the theorem investigated in page 126, the time of a semivibration is $\sqrt{\frac{ap^2}{8lf}}$ the same as is deduced from the more general expression.

Corollary to the solution in page 146.—If the points of quiescence of the auxiliary springs are in the latter semiarcs of vibration, and the vibration commences from the point N (fig. 5.), in this case $d = b$, or $c = 2d$, and by the solution in page 146, the time of a semivibration becomes $\sqrt{\frac{a}{2lf \times \overline{1+2n}}}$ × an arc of which the sine is $\sqrt{\frac{d^2 \times \overline{1+2n}}{d^2 \times \overline{1+2n}}}$, or an arc of $90^\circ = \frac{p}{2}$: wherefore the time of a semivibration $= \sqrt{\frac{ap^2}{8lf \times \overline{1+2n}}}$. We observe, therefore, that whether the points of quiescence of the auxiliary springs are situated in the first or latter semiarcs of vibration, if the semiarc of vibration should be = to the distance of the said points from the point of quiescence of the balance spring, the times of vibration will be the same whatever be the magnitude of that semiarc.

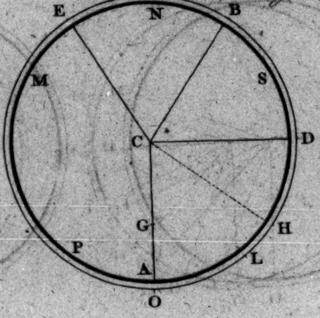
Note to page 164.—Supposing, as in the former examples, the points of quiescence of the auxiliary springs to be at the distance of 1° from the point of quiescence of the balance spring, the variations of rate from mean time, when the semiarcs of vibration are 135°, 125°, 60°, and 10° severally, will be as expressed underneath:

Points of quiescence in the first semiarcs of vibration.

Semiarcs of vibration.	Variation of the daily rate from mean time.
135°	− 19″.40
125°	− 20″.92
60°	− 44″.33
10°	− 4′.24″.24
Points of quiescence in the latter semiarcs of vibration.	
135°	+ 19″.38
125°	+ 20″.91
60°	+ 43″.60
10°	+ 4′.23″.60

If auxiliary springs should be applied for the sole purpose of making the vibrations of the balance isochronal, it may probably be found convenient to adjust the points of quiescence of the auxiliary and balance springs at a greater distance than 1°; suppose the distance to be 10°. Let the arc of vibration be 450°, or 1¼ of a revolution, and suppose the balance to make four vibrations in a second; on these conditions two auxiliary springs, each of which is about $\frac{1}{16}$ part of the force of the balance spring, or perhaps a single auxiliary spring proportionally stronger, will, in most cases, be sufficient to compensate for the want of isochronism in the balance spring.

Fig. 1.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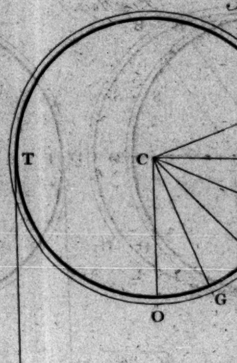
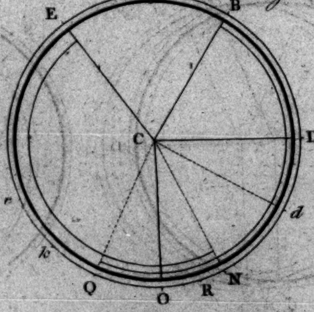


Fig. 5.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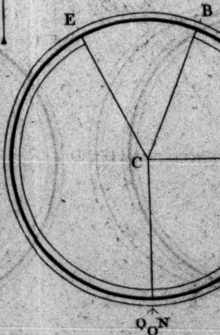

Fig. 2.

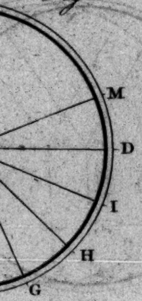

Fig. 3.

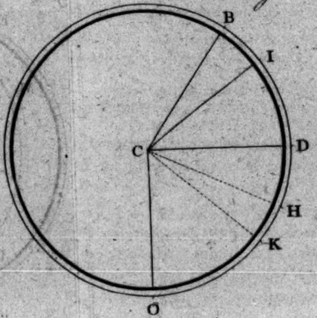

Fig. 4.

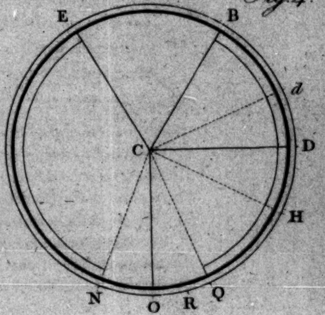
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Fig. 6.

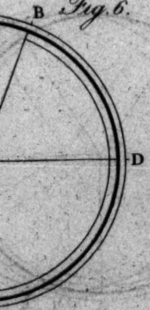

Fig. 7.

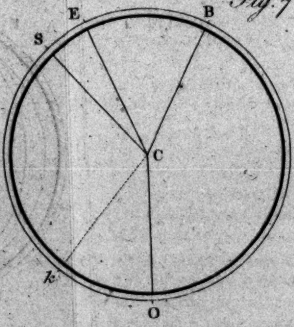

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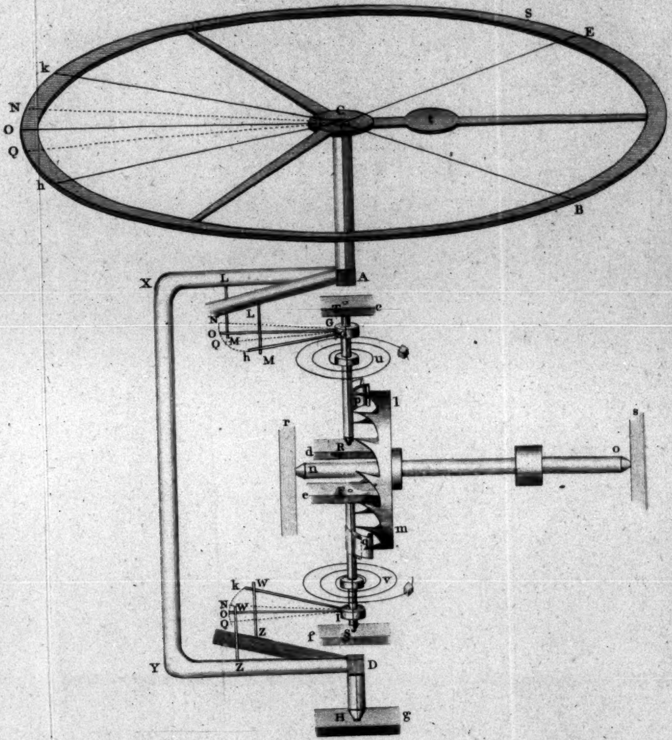
Fig. 8.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METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1793.

1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Jan. 1	8	0	35	50	29,86	72		SE	1	Cloudy.
	2	0	36	52	29,77	72		SSE	1	Cloudy.
2	8	0	35	49	29,77	77	0,127	NNE	1	Foggy.
	2	0	38	51	29,77	75		NW	1	Cloudy.
3	8	0	30	49	29,93	76				Foggy.
	2	0	31,5	52	29,92	75		W	1	Foggy.
4	8	0	31,5	48	29,62	75		E	2	Cloudy.
	2	0	34	49	29,73	73		E	2	Cloudy.
5	8	0	34,5	48,5	29,84	71		SE	2	Cloudy.
	2	0	38	49	29,65	79		SSW	2	Rain.
6	8	0	34	48,5	29,94	74	0,100	W	1	Fair.
	2	0	39,5	50,5	30,06	70		WNW	1	Fine.
7	8	0	31	49	30,21	74		SW	1	Cloudy.
	2	0	39	50,5	30,15	74		E	1	Cloudy.
8	8	0	46	50	29,70	86	0,457	W	1	Rain.
	2	0	45	53	29,88	66		WNW	2	Cloudy.
9	8	0	32	49	30,11	76		WNW	1	Fine.
	2	0	38	51	30,12	75		WNW	1	Fair.
10	8	0	40,5	49	29,85	80		SSE	2	Cloudy.
	2	0	47,5	51,5	29,71	83		S	2	Cloudy.
11	8	0	43,5	51	29,54	79	0,053	SSW	2	Cloudy.
	2	0	46	54	29,56	71		SSW	2	Cloudy.
12	8	0	42	51	28,98	75	0,380	SSW	3	Rain.
	2	0	43	53	29,11	67		WSW	2	Cloudy.
13	8	0	37	50	29,34	76	0,053	SW	1	Cloudy.
	2	0	44	52	29,34	71		SW	1	Cloudy.
14	8	0	38	50,5	29,55	79		NE	1	Cloudy.
	2	0	42	51,5	29,68	72		NE	2	Cloudy.
15	8	0	36	50	30,00	73		NE	2	Cloudy.
	2	0	38	50,5	30,07	69		NE	2	Cloudy.
16	8	0	33,5	48	30,24	68		NE	2	Cloudy.
	2	0	35	48	30,27	64		NE	2	Cloudy.

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for January, 1793.

1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Jan. 17	8	0	36	48	30,25	•		NW	1	Rain.
	2	0	37	50	30,27			NE	2	Cloudy.
18	8	0	33,5	48	30,46			ENE	2	Cloudy.
	2	0	36	48,5	30,47			E	2	Fine.
19	8	0	28	45	30,52			NE	1	Fine.
	2	0	33	47	30,51			NE	1	Hazy.
20	8	0	32	46,5	30,49			WNW	1	Cloudy.
	2	0	34	47	30,50			NW	1	Cloudy.
21	8	0	31	47	30,50			WNW	1	Cloudy.
	2	0	37	48	30,50			W	1	Cloudy.
22	8	0	35	46	30,50			W	1	Foggy.
	2	0	39	49,5	30,52			W	1	Cloudy.
23	8	0	33	47	30,47			W	1	Cloudy.
	2	0	36	48	30,41			SW	1	Cloudy.
24	8	0	39	47	30,22			W	1	Cloudy.
	2	0	43	50	30,15			W	1	Rain.
25	8	0	39	49	30,00		0,378	E	1	Rain.
	2	0	41	51	30,07			NE	1	Cloudy.
26	8	0	31	48	30,23					Foggy.
	2	0	37	50	30,24			E	1	Fair.
27	8	0	33	47,5	30,30			W	1	Cloudy.
	2	0	41	50,5	30,30			W	1	Cloudy.
28	8	0	39	48	30,18			W	1	Cloudy.
	2	0	48	52,5	30,16			W	1	Fine.
29	8	0	40	49	30,00			W	1	Foggy.
	2	0	46	52	29,91			SW	1	Cloudy.
30	8	0	39	50,5	29,80		0,017	WNW	1	Cloudy.
	2	0	43	52	29,82			NW	1	Fine.
31	8	0	36	50	29,88			WNW	2	Fair.
	2	0	44	52	29,87			NW	2	Fair.

• This interruption, in the observations of the hygrometer, was occasioned by one end of the whalebone having slipped out of the pincers by which it is held.

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for February, 1793.

1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Feb. 1	7	0	41,5	50	29,50			S	2	Cloudy.
	2	0	43	52,5	29,37			S	2	Cloudy.
2	7	0	37,5	50	29,59			W	2	Fine.
	2	0	45	53	29,53			SW	2	Cloudy.
3	7	0	43	51,5	29,34		0,246	SW	2	Cloudy.
	2	0	44	54	29,36	61		SW	2	Fair.
4	7	0	39	50,5	29,32	69	0,053	SW	2	Cloudy.
	2	0	43,5	53	29,29	64		SSW	2	Fair.
5	7	0	37	50,5	29,33	72		WNW	1	Foggy.
	2	0	43	55	29,49	67		WNW	1	Fair.
6	7	0	39	51	29,61	74	0,112	W	1	Rain.
	2	0	46	53,5	29,62	62		W	1	Cloudy.
7	7	0	41	50,5	29,57	75	0,278	W	1	Cloudy.
	2	0	43	53,5	29,72	63		WNW	1	Hazy.
8	7	0	38	50,5	29,85	68		WSW	2	Cloudy.
	2	0	43	53	29,60	71		SW	1	Cloudy.
9	7	0	39	50 5	29,78	69	0,290	WNW	1	Cloudy.
	2	0	46	53	29,79	63		W	1	Cloudy.
10	7	0	36	50	29,53	65	0,097	W	2	Fine.
	2	0	40	52	29,52	60		WNW	2	Cloudy.
11	7	0	36,5	49,5	29,58	63	0,037	WNW	2	Fine.
	2	0	42,5	52	29,78	59		WNW	2	Fair.
12	7	0	44	50,5	29,52	63	0,031	NW	2	Cloudy.
	2	0	48	54	29,58	55		NW	2	Cloudy.
13	7	0	39	51	29,92	68		WNW	2	Cloudy.
	2	0	51	54	29,88	74		WSW	1	Cloudy.
14	7	0	47	53	29,67	76		SW	2	Cloudy.
	2	0	44,5	53,5	29,75	64		W	2	Fair.
15	7	0	38,5	52	29,66	75	0,286	SW	2	Rain.
	2	0	41,5	53	29,71	63		SE	1	Cloudy.
16	7	0	35	51	29,78	70		N	2	Cloudy.
	2	0	40,5	53	29,85	65		NNE	2	Fair.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Feb. 17	7	0	34	50,5	30,05	75		NNE	1	Cloudy.
	2	0	41	52,5	30,04	67		NE	1	Cloudy.
18	7	0	37	51	29,80	73		W	1	Fair.
	2	0	43	53	29,77	73		W	1	Cloudy.
19	7	0	32,5	50	30,01	71	0,031	W	1	Fair.
	2	0	43	53	30,05	64		NW	1	Fair.
20	7	0	32	50	30,18	72		WNW	1	Fine.
	2	0	43	54	30,17	64		SW	1	Fine.
21	7	0	37	51	30,11	74		SSW	1	Fine.
	2	0	46	53,5	30,10	64		S	2	Fair.
22	7	0	30	50	30,21	73		S by W	1	Foggy.
	2	0	44	54	30,21	63		SW	1	Fine.
23	7	0	45	52	30,15	80		WSW	1	Cloudy.
	2	0	50	55	30,18	70		WSW	2	Cloudy.
24	7	0	47	53	30,16	77	0,120	W	1	Cloudy.
	2	0	52	57	30,22	65		SWb.S	2	Cloudy.
25	7	0	43,6	53	30,05	78		SWb.S	2	Cloudy.
	2	0	47	57	29,85	72		SWb.S	2	Cloudy.
26	7	0	40	53	29,95	67		WNW	2	Fine.
	2	0	47	56	30,13	52		NW	2	Fair.
27	7	0	42,5	54	30,02	64		SW	2	Cloudy.
	2	0	50	56	29,97	58		SSW	2	Fair.
28	7	0	42,5	54	29,87	72		SW	2	Cloudy.
	2	0	48	56,5	29,96	58		W	2	Fair.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Mar. 1	7	0	43	54,5	29,73	68		SSW	2	Fair.
	2	0	46	56	29,52	70		SSW	2	Cloudy.
2	7	0	42	54	29,70	67	0,095	W	1	Cloudy.
	2	0	51	56,5	29,69	64		SW	2	Cloudy.
3	7	0	41	54	29,62	60	0,063	W	2	Fine.
	2	0	47	56	29,77	51		WNW	2	Fine.
4	7	0	35	53	29,88	68		WNW	2	Fine.
	2	0	45	56	29,89	55		WNW	2	Fair.
5	7	0	35	53	29,78	67		E	1	Cloudy.
	2	0	46	55	29,76	60		ESE	2	Cloudy.
6	7	0	39	53	29,91	70		N	2	Fine.
	2	0	46,5	55,5	29,96	62		NNE	2	Cloudy.
7	7	0	35,5	52	30,02	72		NNE	1	Fine.
	2	0	44	55	30,05	59		NE	2	Fair.
8	7	0	37	52	30,19	60		E	2	Cloudy.
	2	0	42	56	30,21	51		E	2	Fine.
9	7	0	37	51	30,12	61		E	2	Cloudy.
	2	0	41	53,5	30,05	55		E	2	Fair.
10	7	0	34	51,5	29,96	61		ENE	1	Fair
	2	0	40,5	53	29,92	57		E	2	Fair.
11	7	0	36	50	29,76	60		E	2	Cloudy.
	2	0	37,5	52	29,74	58		E	2	Cloudy.
12	7	0	33,5	49,5	29,86	57		E	2	Cloudy.
	2	0	36	51	29,90	56		E	2	Fine.
13	7	0	45	50,5	29,80	83	0,100	SW	2	Cloudy.
	2	0	51	54	29,93	74		SSW	2	Cloudy.
14	7	0	43	52	30,13	80		SW	1	Cloudy.
	2	0	51	55	30,15	55		SSW	1	Fair.
15	7	0	36	53	30,09	72		SWb.W	1	Cloudy.
	2	0	49	57,5	30,02	56		SSW	1	Fine.
16	7	0	44	55	29,86	71		SSW	2	Cloudy.
	2	0	49	58	29,60	57		S	2	Cloudy.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Mar. 17	7	0	35,5	55	29,62	69	0,183	WSW	1	Fine.
	2	0	45,5	56	29,42	63		ESE	1	Hazy.
18	7	0	44	55,5	29,07	80	0,241	S	1	Cloudy.
	2	0	49	57,5	29,20	62		S	2	Cloudy.
19	7	0	38,5	54,5	29,70	68	0,015	WSW	1	Cloudy.
	2	0	48,5	57	29,78	60		W	1	Cloudy.
20	7	0	46	54,5	29,84	76		SW	1	Cloudy.
	2	0	52	58	29,82	65		SSW	2	Cloudy.
21	7	0	47	55,5	29,84	75	0,020	WSW	1	Cloudy.
	2	0	48	58	29,92	65		SW	1	Cloudy.
22	7	0	42	55	29,72	70		SSE	2	Cloudy.
	2	0	43	56,5	29,60	72		S	2	Rain.
23	7	0	37	54,5	29,78	70	0,325	W	1	Fair.
	2	0	46	57	29,78	53		SSW	1	Fair.
24	7	0	36	54	29,90	66		NE	1	Fine.
	2	0	43	55,5	29,91	67		NE	2	Cloudy.
25	7	0	41,5	53	29,98	74	0,023	ENE	2	Cloudy.
	2	0	44,5	55	30,01	64		NE	2	Cloudy.
26	7	0	36,5	52	29,94	78	0,097	NE	2	Rain.
	2	0	40	52	29,91	68		NE	2	Cloudy.
27	7	0	34,5	50,5	30,02	61		NE	2	Cloudy.
	2	0	39	53	29,97	56		NE	2	Cloudy.
28	7	0	34	50,5	29,76	62		NE	1	Cloudy.
	2	0	41	55	29,50	51		E	2	Fair.
29	7	0	33	50,5	29,83	65		NE	2	Cloudy.
	2	0	41,5	54	29,86	51		NE	2	Fair.
30	7	0	33	50	29,98	60		NE	1	Fine.
	2	0	43,5	54,5	29,98	51		NW	1	Fair.
31	7	0	33	51	29,99	67		N	1	Fair.
	2	0	42	53,5	29,94	52		N	1	Fine.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Apr. 1	7	0	36	50	29,77	64	0,025	SSE	2	Cloudy.
	2	0	36,5	51,5	29,63	70		E	1	Rain.
2	7	0	36	50	29,56	75	0,383	WNW	1	Snow.
	2	0	40,5	52	29,68	70		WNW	1	Cloudy.
3	7	0	35	50,5	29,88	73		W	1	Fair.
	2	0	48	54.5	29,91	51		SW	1	Fair.
4	7	0	39	51	30,00	71		NNE	1	Cloudy.
	2	0	51,5	54	30,00	58		N	1	Cloudy.
5	7	0	42	52	30,00	70		NNW	1	Hazy.
	2	0	52	57	30,00	60		N	1	Fine.
6	7	0	41	53.5	30,04	73		NNW	1	Cloudy.
	2	0	54	58	30,04	64		NNE	1	Fine.
7	7	0	40	54.5	30,16	67		NNE	1	Fine.
	2	0	51	58	30,19	56		E	1	Fine.
8	7	0	40	54	30,30	66		ENE	1	Hazy.
	2	0	50,5	57	30,30	47		E	1	Fine.
9	7	0	39	53.5	30,27	66		E	1	Fine.
	2	0	49	59	30,24	47		E	2	Fine.
10	7	0	37	53	30,21	65		ENE	2	Fine.
	2	0	46	55.5	30,16	49		NE	2	Cloudy.
11	7	0	37	53	30,08	64		NNW	1	Fair.
	2	0	45	54.5	29.96	64		NW	1	Cloudy.
12	7	0	39.5	53	29,88	68		NNE	1	Cloudy.
	2	0	44	54	29,88	61		NE	1	Cloudy.
13	7	0	37	52	29,76	68	0,040	NE	1	Fine.
	2	0	49	56	29.70	54		NE	1	Fair.
14	7	0	43	53.5	29.74	67		W	1	Cloudy.
	2	0	46	55	29,82	57		NNE	2	Cloudy.
15	7	0	33	52	29.94	59		W	1	Cloudy.
	2	0	50	54	29,81	52		W	2	Cloudy.
16	7	0	38	53	29.64	66	0,038	NW	2	Cloudy.
	2	0	45	54	29.76	47		N	2	Cloudy.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Apr. 17	7	0	34	51	30,07	63		W	1	Fine.
	2	0	40	52	30,00	60		SSW	2	Rain.
18	7	0	47	52	29,27	85	0,543	WSW	1	Cloudy.
	2	0	53,5	54,5	29,22	63		W	2	Cloudy.
19	7	0	42	53,5	29,58	63	0,066	NW	2	Cloudy.
	2	0	43	54,5	29,86	64		N	2	Cloudy.
20	7	0	36	53	30,19	66		NW	1	Fair.
	2	0	48	55,5	30,22	52		N	1	Fair.
21	7	0	42	53	30,15	57		SSE	1	Cloudy.
	2	0	54,5	57	30,15	51		S	1	Fine.
22	7	0	43	54	30,20	60		E	1	Fine.
	2	0	54,5	57	30,18	46		E	1	Fine.
23	7	0	44	55	29,88	63		E	1	Fine.
	2	0	59	58,5	29,79	48		ESE	1	Hazy.
24	7	0	48	56,5	29,70	63		NE	1	Cloudy.
	2	0	60	59,5	29,70	53		E	1	Fine.
25	7	0	47	57,5	29,76	70		N	1	Cloudy.
	2	0	53	58	29,79	60		N	1	Cloudy.
26	7	0	47	57,5	29,85	67		W	1	Cloudy.
	2	0	58	59	29,78	53		SSW	1	Cloudy.
27	7	0	47	57	29,57	63		E	1	Cloudy.
	2	0	56	58,5	29,55	56		E	1	Cloudy.
28	7	0	46	57	29,75	62		NE	1	Cloudy.
	2	0	57	59,5	29,80	55		NW	1	Hazy.
29	7	0	45	58	29,88	65		S	1	Fair.
	2	0	52,5	59,5	29,77	60		S	2	Cloudy.
30	7	0	46	57,5	29,77	66		WSW	2	Fine.
	2	0	57	59,5	29,68	54		SW	2	Cloudy.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
May 1	7	0	48	58,5	29,27	66	0,090	SW	2	Cloudy.
	2	0	57	59,5	29,24	57		SW	2	Cloudy.
2	7	0	48	57	29,36	65	0,065	SW	2	Cloudy.
	2	0	53,5	61	29,50	64		WSW	2	Cloudy.
3	7	0	44	57,5	30,04	68	0,284	WNW	1	Fine.
	2	0	56	59	30,15	60		WNW	1	Fair.
4	7	0	45	57	30,29	68		W	1	Fair.
	2	0	54	59	30,25	52		WSW	1	Cloudy.
5	7	0	50	58	29,90	65		W	1	Cloudy.
	2	0	54	58	30,07	49		NW	1	Cloudy.
6	7	0	45	57	30,29	61		NW	1	Fine.
	2	0	59	59,5	30,28	49		NW	1	Cloudy.
7	7	0	47	57,5	30,17	64		N	1	Cloudy.
	2	0	53,5	58	30,15	60		N	1	Cloudy.
8	7	0	44	58	30,14	68	0,100	E	1	Cloudy.
	2	0	58	59,5	30,08	56		SSE	1	Cloudy.
9	7	0	47	58	30,05	70	0,165	SW	1	Cloudy.
	2	0	61	61,5	30,11	50		WSW	1	Fair.
10	7	0	48	59	30,18	64		W	1	Cloudy.
	2	0	55,5	59,5	30,06	57		SSE	1	Cloudy.
11	7	0	52	59	29,78	64		E	1	Fair.
	2	0	60	60	29,77	56		E	1	Hazy.
12	7	0	55,5	59,5	29,89	68		NNE	1	Fine.
	2	0	66	60	29,93	60		E	1	Fine.
13	7	0	53	60	29,92	67		NE	1	Cloudy.
	2	0	69	64	29,93	53		NNE	1	Fine.
14	7	0	51	61	30,11	61		N	1	Fine.
	2	0	61	62	30,16	47		NNE	1	Fine.
15	7	0	52	59	30 14	58		NW	1	Hazy.
	2	0	63,5	60,5	30,17	55		NW	1	Cloudy.
16	7	0	52	58	30,26	58		N	1	Fine.
	2	0	61	59,5	30,23	47		NW	1	Fine.

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	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
May 17	7	0	55	60,5	30,09	65		WNW	1	Cloudy.
	2	0	65	62	29,93	60		WNW	1	Cloudy.
18	7	0	52	61	29,98	65	0,161	NE	1	Cloudy.
	2	0	61	61	30,01	58		NNE	1	Cloudy.
19	7	0	48	59,5	30,06	59		N	1	Cloudy.
	2	0	54	59	30,06	58		NNE	2	Cloudy.
20	7	0	48	57	30,11	62		NNE	1	Fair.
	2	0	61	60,5	30,10	49		NE	1	Fine.
21	7	0	50	59	30,15	61		N	1	Fine.
	2	0	65,5	61,5	30,15	50		N	1	Fine.
22	7	0	49	59,5	30,23	56		N	2	Cloudy.
	2	0	57	59,5	30,23	55		N	2	Cloudy.
23	7	0	51	57,5	30,22	61		N	2	Fair.
	2	0	63	60	30,22	55		E	1	Fine.
24	7	0	50	59	30,21	64		N	1	Cloudy.
	2	0	57	61	30,18	52		NW	1	Fair.
25	7	0	48	58	30,26	60		N	1	Fine.
	2	0	61	59	30,26	45		N	1	Fine.
26	7	0	51	58,5	30,29	49		NNE	1	Fine.
	2	0	63	61	30,25	45		NNE	1	Cloudy.
27	7	0	49	58	30,22	60		N	1	Cloudy.
	2	0	69	59,5	30,12	50		NW	1	Hazy.
28	7	0	52	59	29,97	64		WNW	1	Cloudy.
	2	0	64	60	29,88	51		NW	1	Fair.
29	7	0	48	58,5	29,85	58		W	1	Cloudy.
	2	0	53	58	29,85	45		WNW	1	Fair.
30	7	0	46	57	29,82	55		W	2	Fine.
	2	0	58	57	29,91	43		NW	2	Fine.
31	7	0	46	56	30,13	50		NW	1	Fine.
	2	0	58	58	30,15	45		NW	1	Cloudy.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
June 1	7	0	47	57	30,16	56		WSW	1	Fine.
	2	0	65	58,5	30,05	46		SW	2	Hazy.
2	7	0	54	57	29,82	52		SE b. E	2	Cloudy.
	2	0	63	58	29,80	48		SE	2	Cloudy.
3	7	0	54	58	29,94	63		SW	1	Fair.
	2	0	67,5	61	30,01	50		S by W	2	Fair.
4	7	0	58	59,5	30,09	55		S by W	2	Fair.
	2	0	70	61,5	30,05	49		S by W	2	Cloudy.
5	7	0	57,5	60,5	30,05	58		E	1	Hazy.
	2	0	67	62	30,05	54		SSW	1	Cloudy.
6	7	0	56	61,5	30,02	60		SSW	1	Fair.
	2	0	64	63	30,00	57		SSW	2	Cloudy.
7	7	0	56	61,5	29,93	66		WSW	2	Cloudy.
	2	0	70	63	29,89	48		WSW	1	Fine.
8	7	0	55	62	29,86	62		WSW	2	Cloudy.
	2	0	65	63	29,91	57		SWb.W	2	Cloudy.
9	7	0	54	62	30,01	60		SSW	2	Fair.
	2	0	65	62,5	30,01	54		SSW	2	Cloudy.
10	7	0	52	61,5	30,09	60		W	2	Fair.
	2	0	67,5	63	30,10	48		W	2	Fair.
11	7	0	52	61	30,10	64		W	1	Fair.
	2	0	64	62	30,04	51		SW	1	Fair.
12	7	0	52	61,5	29,94	59		WSW	1	Fine.
	2	0	62	62	30,00	53		NW	1	Cloudy.
13	7	0	54	61	30,13	60		NW	1	Hazy.
	2	0	59	61	30,15	52		NE	2	Cloudy.
14	7	0	50	59,5	30,20	60		NNE	1	Cloudy.
	2	0	57	60	30,19	55		N	1	Cloudy.
15	7	0	50	59	30,14	58		N	1	Cloudy.
	2	0	61	60	30,15	52		WNW	1	Fair.
16	7	0	54	59,5	30,12	58		NW	1	Fair.
	2	0	65	60	30,07	52		NW	1	Cloudy.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
June 17	7	0	57	60	29,86	63	0,030	W	1	Cloudy.
	2	0	70	61,5	29,83	50		WNW	2	Fair.
18	7	0	52	60	29,94	54		NW	2	Fine.
	2	0	63	60	29,96	50		NW	1	Fair.
19	7	0	54	60	29,71	65	0,198	W	1	Rain.
	2	0	60	60	29,68	64		NW	1	Cloudy.
20	7	0	52	59,5	29,90	60	0,088	NW	1	Cloudy.
	2	0	55	58,5	29,97	57		NW	1	Cloudy.
21	7	0	52	59	30,07	56		E	1	Cloudy.
	2	0	66	60	30,04	50		W	1	Fair.
22	7	0	52	59	29,78	59		WNW	1	Cloudy.
	2	0	62	60,5	29,79	50		WNW	1	Cloudy.
23	7	0	50	59	29,95	54		WNW	1	Fine.
	2	0	64	61	29,98	47		WNW	1	Fine.
24	7	0	51	59	29,97	59		W	1	Cloudy.
	2	0	58	59,5	29,91	60		W	1	Cloudy.
25	7	0	56	59,5	29,88	63	0,025	W	1	Fair.
	2	0	63	60	29,91	55		SW	1	Cloudy.
26	7	0	58	60,5	29,78	72	0,066	WSW	1	Cloudy.
	2	0	72	62,5	29,78	47		WNW	1	Fair.
27	7	0	54	60,5	29,73	63		W	1	Fine.
	2	0	70	62	29,81	48		W	1	Fine.
28	7	0	57	61,5	29,84	65		WSW	1	Fair.
	2	0	67	62	29,90	48		SW	2	Cloudy:
29	7	0	57	62	29,80	65		S	2	Cloudy.
	2	0	65	62	29,73	63		SSW	2	Cloudy.
30	7	0	57	62,5	29,90	61	0,020	SW	2	Fine.
	2	0	64	63	29,90	56		SW	2	Cloudy.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
July 1	7	0	57	62,5	29,85	62		SW	2	Cloudy.
	2	0	67	63	29,85	63		SW	2	Cloudy.
2	7	0	56	62	30,04	62	0,070	WSW	2	Fair.
	2	0	68	63	30,09	49		W	2	Fair.
3	7	0	54	62	30,23	61		W	1	Fine.
	2	0	74	63	30,22	48		W	1	Cloudy.
4	7	0	57	62	30,18	56		SW	2	Cloudy.
	2	0	71	63,5	30,12	49		SW	2	Fair.
5	7	0	60	64	30,16	61		WSW	2	Fine.
	2	0	80,5	66,5	30,20	48		W	2	Fine.
6	7	0	60	66	30,30	66		W	2	Fair.
	2	0	86	68,5	30,26	51		SSW	1	Fine.
7	7	0	65	69	30,14	62		SW	1	Fine.
	2	0	89	72	30,08	48		SW b.S	1	Fine.
8	7	0	66	72	30,09	61		E	1	Hazy.
	2	0	80	73,5	30,08	52		E	1	Fine.
9	7	0	62	71	30,10	62		E	2	Cloudy.
	2	0	74	72,5	30,08	54		E	1	Fine.
10	7	0	66	71	30,07	59		E	1	Hazy.
	2	0	82,5	73	30,07	43		E	1	Fine.
11	7	0	67	72	30,01	56		E	1	Cloudy.
	2	0	80	74,5	29,96	49		E	1	Fine.
12	7	0	66	72	29,98	56		E	1	Fine.
	2	0	83	76	30,00	45		E	1	Fine.
13	7	0	65	73,5	30,10	58		NE	1	Hazy.
	2	0	82	76	30,11	46		E	1	Fine.
14	7	0	62	73	30,18	53		NE	1	Hazy.
	2	0	79	76	30,16	47		E	1	Fine.
15	7	0	65	73	30,17	50		NE	1	Hazy.
	2	0	83,5	75	30,10	43		E	1	Fine.
16	7	0	66	73,5	29,96	50		E	1	Hazy.
	2	0	89	76	29,85	45		SW	1	Fine.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
July 17	7	0	65	74	29,82	53		SSW	1	Fine.
	2	0	80	74	29,88	49		WSW	1	Fair.
18	7	0	62	73	29,85	55	0,049	SW	1	Cloudy.
	2	0	67	72	29,76	53		WSW	1	Cloudy.
19	7	0	60	70	29,76	58	0,064	WSW	2	Cloudy.
	2	0	68	70	29,74	47		SSW	2	Cloudy.
20	7	0	57	68	29,85	57		WNW	1	Cloudy.
	2	0	65	68	29,93	53		NW	1	Cloudy.
21	7	0	56	67	30,20	56	0,070	NNW	1	Cloudy.
	2	0	67	67	30,21	48		NW	1	Cloudy.
22	7	0	56	66	30,21	58		S	2	Fair.
	2	0	73,5	67	30,14	48		S by W	2	Hazy.
23	7	0	60	66	30,01	60		SSW	2	Cloudy.
	2	0	63	66,5	29,98	59		SSW	2	Rain.
24	7	0	64	66	30,00	62	0,011	WSW	1	Cloudy.
	2	0	78	69	30,01	51		W	1	Cloudy.
25	7	0	65	68	30,01	65		W	1	Cloudy.
	2	0	75	69	30,01	56		W	1	Fair.
26	7	0	63	68,5	30,06	60		W	1	Cloudy.
	2	0	75	70	30,06	52		W	1	Cloudy.
27	7	0	64	69	30,00	64		E	1	Cloudy.
	2	0	63	69	29,98	69		ENE	1	Rain.
28	7	0	59	67,5	29,92	74	1,137	NNE	1	Cloudy.
	2	0	64	67	29,91	59		N	1	Cloudy.
29	7	0	56	65	29,91	65		NW	1	Cloudy.
	2	0	67,5	66	29,92	53		NW	1	Fair.
30	7	0	56	65,5	29,97	65		WNW	1	Fine.
	2	0	71	66,5	29,96	52		SW	2	Fair.
31	7	0	60	66	29,91	68	0,215	SSW	2	Rain.
	2	0	71	67	29,95	51		SW	2	Fair.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Aug. 1	7	0	58	66	30,16	66		W	1	Fine.
	2	0	74,5	67	30,18	49		W	1	Fine.
2	7	0	61	66,5	30,17	61		N	1	Fine.
	2	0	75	68,5	30,08	51		E	1	Fair.
3	7	0	64	67	29,86	68		E	2	Fair.
	2	0	78,5	70,5	29,78	58		SSE	2	Fair.
4	7	0	62	68,5	29,70	67	0,358	WSW	1	Cloudy.
	2	0	65	68,5	29,71	53		S	2	Cloudy.
5	7	0	60	67,5	29,92	66		W	2	Cloudy.
	2	0	72,5	69	29,85	54		SSW	2	Fair.
6	7	0	58	67	30,06	65	0,158	W	2	Fine.
	2	0	70,5	68	30,14	50		WNW	2	Fair.
7	7	0	59	67,5	30,17	64		E	1	Fine.
	2	0	68	68	30,03	53		E	2	Cloudy.
8	7	0	60	67	29,84	73	0,257	SW	2	Cloudy.
	2	0	73	68	29,81	50		SW	2	Fair.
9	7	0	60	66	29,80	63		SW	2	Cloudy.
	2	0	67	67	29,80	53		SW	2	Fair.
10	7	0	62	66	29,80	59		SW	2	Cloudy.
	2	0	67	67	29,88	53		SW	2	Fair.
11	7	0	56	66	30,08	64		SW	1	Fine.
	2	0	69	66,5	30,06	47		SW	1	Hazy.
12	7	0	59	66	30,07	60		WSW	1	Cloudy.
	2	0	74	68	30,04	49		S	1	Fine.
13	7	0	63	67	29,92	58		E	1	Cloudy.
	2	0	80	70	29,85	51		SSW	2	Fair.
14	7	0	56	67	30,03	61		WSW	2	Fine,
	2	0	72	68	29,96	47		WSW	1	Cloudy.
15	7	0	58,5	67	29,69	61		S	1	Cloudy.
	2	0	68	67	29,44	57		S	2	Cloudy.
16	7	0	57	65,5	29,63	63		W	2	Fine.
	2	0	68	67	29,72	49		W	2	Fair.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	o	o	Inches.		Inches.	Points.	Str.	
Aug. 17	7	0	57	65	29,58	66		E	1	Cloudy.
	2	0	58	65,5	29,34	63		S	2	Rain.
18	7	0	54	64	29,73	66	0,460	WNW	2	Cloudy.
	2	0	61	64,5	29,94	57		WNW	2	Cloudy.
19	7	0	52	63,5	30,10	67		WSW	1	Cloudy.
	2	0	67	65	30,10	55		S	1	Fair.
20	7	0	53	63,5	30,18	65		S	1	Fine.
	2	0	69,5	65	30,20	50		S	1	Fine.
21	7	0	58	64	30,28	68		S	1	Fine.
	2	0	74	66,5	30,28	54		S	1	Fine.
22	7	0	57	65	30,21	68		S	1	Hazy.
	2	0	75,5	67	30,13	52		SW	1	Fair.
23	7	0	62	66	30,01	68		W	1	Cloudy.
	2	0	71,5	68	30,01	50		SW	1	Hazy.
24	7	0	55	65,5	30,09	63		SW	1	Fair.
	2	0	67	66,5	30,07	50		SW	1	Hazy.
25	7	0	54	65	30,18	62		SW	1	Fine.
	2	0	65,5	65	30,18	51		E	1	Hazy.
26	7	0	53	64	30,10	64		NE	1	Hazy.
	2	0	68,5	67	30,00	50		SW	1	Hazy.
27	7	0	53	64,5	29,94	63		WSW	1	Fine.
	2	0	67	65	29,94	48		WNW	1	Cloudy.
28	7	0	55	64	30,04	67		W	1	Hazy.
	2	0	67,5	65	30,14	53		W	1	Hazy.
29	7	0	57	64	30,05	60		NE	1	Cloudy.
	2	0	68,5	66	29,99	52		SSW	1	Fine.
30	7	0	57,5	65	29,80	66		SSW	1	Cloudy.
	2	0	66	66	29,73	60		SSW	2	Cloudy.
31	7	0	56	65	29,77	63	0,082	NW	2	Cloudy.
	2	0	65	65	29,75	56		W	2	Fair.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Sep. 1	7	0	53	64	29,87	64		WSW	2	Cloudy.
	2	0	56	64	29,71	69		SSW	2	Rain.
2	7	0	55	63,5	29,66	71	0,309	WSW	1	Cloudy.
	2	0	64	64,5	29,59	59		W	2	Fair.
3	7	0	56	63	29,58	61		WNW	2	Fair.
	2	0	66	64.5	29,76	60		WNW	2	Fair.
4	7	0	48	62,5	29,98	63		WNW	2	Fair.
	2	0	64	63	30,05	50		WNW	2	Fair.
5	7	0	46	62	30,09	63		WNW	1	Fine.
	2	0	59	62	30,08	53		WNW	1	Fair.
6	7	0	49	61	30,07	61		W	1	Cloudy.
	2	0	60,5	63	30,05	59		W	1	Cloudy.
7	7	0	42	59	30,20	62	0,030	W	1	Fine.
	2	0	60	62	30,26	51		W	1	Fine.
8	7	0	54	61	30,31	74		W	1	Cloudy.
	2	0	66	62	30,32	59		W	1	Cloudy.
9	7	0	54	61	30,31	67		W	1	Cloudy.
	2	0	63	62	30,26	56		W	1	Cloudy.
10	7	0	53	61	30,11	67		W	1	Cloudy.
	2	0	66	62	30,10	59		W	1	Cloudy.
11	7	0	48	61,5	30,10	67		W	1	Foggy.
	2	0	62	62	30,05	62		W	1	Fair.
12	7	0	53	62	30,02	68		E	1	Foggy.
	2	0	66,5	64	29,95	60		E	1	Fine.
13	7	0	59	63	29,88	73	0,153	W	1	Cloudy.
	2	0	64	64	29,83	66		SW	1	Rain.
14	7	0	57	63	29,79	72	0,260	SSE	1	Fair.
	2	0	65,5	64,5	29,71	61		SSE	2	Cloudy.
15	7	0	57	64	29,41	80	0,680	SW	1	Rain.
	2	0	63,5	66	29,53	58		SW	1	Fair.
16	7	0	52	63	29,59	71	0,198	WNW	1	Fair.
	2	0	62	64	29,68	61		WNW	1	Fair.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Sep. 17	7	0	49	62,5	29,96	72		WSW	1	Fine.
	2	0	62,5	63,5	29,98	60		S	2	Cloudy.
18	7	0	60	63,5	29,77	82	0,277	SSW	1	Cloudy.
	2	0	64	64	29,78	76		SSW	1	Cloudy.
19	7	0	53	62,5	30,12	71	0,120	NE	1	Cloudy.
	2	0	57	62	30,16	63		NE	1	Cloudy.
20	7	0	52	61	30,10	72		NE	1	Cloudy.
	2	0	58	62	30,13	57		NE	1	Fair.
21	7	0	46	60	30,18	67	0,040	ENE	2	Fine.
	2	0	51	61	30,17	65		NE	2	Cloudy.
22	7	0	46	59	29,88	70	0,047	NE	2	Cloudy.
	2	0	51,5	58	29,72	69		NE	1	Cloudy.
23	7	0	44	57	29,55	71	0,099	SE	2	Cloudy.
	2	0	48	57	29,62	65		S	2	Rain.
24	7	0	46	56	29,98	73	0,098	SW	1	Cloudy.
	2	0	58	57,5	30,06	61		SSW	1	Cloudy.
25	7	0	47	56,5	30,22	75	0,141	NW	1	Fair.
	2	0	59	58,5	30,28	59		E	1	Fine.
26	7	0	44	57	30,45	73		ENE	1	Cloudy.
	2	0	58	59	30,45	62		E	1	Fine.
27	7	0	45	57	30,44	74		E	1	Fine.
	2	0	58	59	30,36	62		E	1	Fine.
28	7	0	49	58,5	30,20	77		ENE	1	Fair.
	2	0	59,5	60,5	30,07	62		E	1	Fine.
29	7	0	50	58,5	29,80	74		E	1	Cloudy.
	2	0	59	60	29,71	61		SE	1	Cloudy.
30	7	0	52	58,5	29,71	72		SSW	1	Cloudy.
	2	0	58	59,5	29,77	66		SW	1	Cloudy.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Oct. 1	7	0	58	59,5	29,55	84	0,118	SSW	2	Rain.
	2	0	61	60	29,52	79		SSW	2	Cloudy.
2	7	0	49	58,5	29,71	69	0,035	WSW	2	Fine.
	2	0	55	60	29,79	59		SW	2	Cloudy.
3	7	0	49	58,5	30,08	73	0,053	SW	2	Cloudy.
	2	0	59,5	59,5	30,14	60		WSW	2	Cloudy.
4	7	0	56	59	30,02	76		SW	2	Cloudy.
	2	0	65	62	30,08	65		SW	2	Fine.
5	7	0	58	61	30,20	77		SW	2	Cloudy.
	2	0	64,5	62,5	30,23	65		W	2	Fair.
6	7	0	57	62	30,20	74		WSW	2	Cloudy.
	2	0	65	62	30,21	64		SW	1	Cloudy.
7	7	0	49	61	30,28	74		SW	1	Cloudy.
	2	0	60,5	62,5	30,22	60		SW	1	Fine.
8	7	0	50	61,5	30,26	73		SSW	1	Fine.
	2	0	61,5	61,5	30,25	61		SW	1	Cloudy.
9	7	0	53	61	30,18	74		SW	1	Cloudy.
	2	0	62	63	30,12	68		SW	1	Fine.
10	7	0	51	61	29,95	74				Foggy.
	2	0	63,5	63,5	29,88	70		SW	1	Fine.
11	7	0	53	62	29,90	69		SW	1	Cloudy.
	2	0	58	62	29,95	63		SW	1	Cloudy.
12	7	0	55	61,5	29,91	78		E	1	Foggy.
	2	0	65	63	29,91	70		E	1	Fine.
13	7	0	57	62	30,03	76		SSE	1	Fine.
	2	0	63,5	64	30,07	59		SSE	1	Fine.
14	7	0	57	62	30,07	71		E	1	Cloudy.
	2	0	61	63	30,19	61		E	1	Fair.
15	7	0	42	61	30,47	62		S	1	Fine.
	2	0	57	62	30,48	61		E	1	Fine.
16	7	0	53	62	30,48	67		NNW	1	Cloudy.
	2	0	61	63	30,38	60		NW	1	Fair.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Oct. 17	7	0	50	61	30,28	72		WNW	1	Fair.
	2	0	59	61,5	30,25	66		NNE	1	Cloudy.
18	7	0	48	60,5	30,28	71		SSE	1	Cloudy.
	2	0	55	60,5	30,26	70		NW	1	Cloudy.
19	7	0	52	60	30,21	67		W	1	Cloudy.
	2	0	54	60	30,18	62		W	1	Cloudy.
20	7	0	52	59	30,13	66		W	1	Cloudy.
	2	0	54	59,5	30,12	61		W	1	Cloudy.
21	7	0	47,5	58	30,13	72		WNW	1	Fine.
	2	0	60	60	30,13	60		NW	1	Fine.
22	7	0	53	59	30,18	70		SW	1	Cloudy.
	2	0	55	60	30,22	69		E	1	Cloudy.
23	7	0	50	59	30,27	65		E	1	Cloudy.
	2	0	56	60	30,21	57		E	1	Fair.
24	7	0	45	59	30,04	61		E	1	Fine.
	2	0	52	60	29,92	58		S by W	1	Cloudy.
25	7	0	49	59	29,76	70		SSW	1	Cloudy.
	2	0	53,5	61	29,65	67		S by W	1	Rain.
26	7	0	57	60	29,53	74		SSW	2	Cloudy.
	2	0	58	62	29,50	71		SSW	2	Cloudy.
27	7	0	52	61	29,47	64		SSW	1	Cloudy.
	2	0	57	62	29,47	66		SSW	1	Cloudy.
28	7	0	43	58	29,75	73		WNW	1	Fine.
	2	0	53	62	29,94	60		WNW	1	Fine.
29	7	0	48	59	29,78	70		SW	1	Cloudy.
	2	0	56	60	29,53	60		SW	2	Cloudy.
30	7	0	39	58	29,29	72	0,931	WNW	1	Fine.
	2	0	47	59	29,36	59		WNW	1	Fine.
31	7	0	35,5	56	29,22	73		WNW	1	Fine.
	2	0	44	57,5	29,31	64		WNW	1	Fine.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	o	o	Inches.		Inches.	Points.	Str.	
Nov. 1	7	0	37	55	29,58	74		NW	1	Cloudy.
	2	0	43	56	29,68	66		NE	1	Fair.
2	7	0	31	53,5	29,93	70		W	1	Fine.
	2	0	46	56	29,90	65		SE	1	Fair.
3	7	0	46	54	29,60	78	0,326	WSW	1	Cloudy.
	2	0	50,5	57	29,65	61		WSW	1	Fine.
4	7	0	39	54	29,63	70		E	1	Cloudy.
	2	0	45	54,5	29,43	68		E	1	Cloudy.
5	7	0	35	53	29,60	74	0,391	W	1	Fine.
	2	0	46	56	29,65	66		WNW	1	Fine.
6	7	0	39	53,5	29,82	75		NE	1	Fine.
	2	0	47	56	29,90	66		NE	1	Fine.
7	7	0	38	53	29,96	73		NW	1	Fair.
	2	0	45	56	29,96	66		N	1	Cloudy.
8	7	0	36	53	29,90	73				Foggy.
	2	0	44	54	29,75	70		E	1	Cloudy.
9	7	0	38	53	29,50	77	0,277	W	1	Fair.
	2	0	48	55	29,48	71		SE	1	Fair.
10	7	0	48.	53,5	29,05	83	0,414	W	1	Rain.
	2	0	49,5	57	29,21	68		SW	1	Fine.
11	7	0	45	54	29,19	80	0,093	WNW	1	Cloudy.
	2	0	50	57	29,38	64		WNW	1	Fair.
12	7	0	36	54	29,69	74		NE	1	Fine.
	2	0	48	55	29,53	76		E	1	Cloudy.
13	7	0	48	54,5	29,27	74	0,310	SW	2	Fair.
	2	0	52	56,5	29,28	66		SSW	2	Cloudy.
14	7	0	40	55	29,43	74		SSW	1	Cloudy.
	2	0	54	57	29,49	64		SSW	1	Cloudy.
15	7	0	42	55	29,51	76		E	1	Cloudy.
	2	0	52	57	29,48	70		ESE	1	Cloudy.
16	7	0	46	56	29,44	75		E	1	Cloudy.
	2	0	48	57	29,48	72		E	1	Cloudy.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Nov. 17	7	0	47	57	29,67	83	0,236	E	1	Cloudy.
	2	0	50	58	29,66	78		E	1	Rain.
18	7	0	50	56,5	29,59	75	0,057	E	1	Cloudy.
	2	0	56	59	29,65	71		E	1	Cloudy.
19	7	0	47	58	29,57	80		E	1	Foggy.
	2	0	56	60	29,59	70		E	1	Fine.
20	7	0	45	58	29,82	78		ENE	1	Cloudy.
	2	0	52	59,5	29,86	75		E	1	Cloudy.
21	7	0	47	58	30,01	75		E	1	Cloudy.
	2	0	52	59,5	30,01	67		E	1	Fair.
22	7	0	43	57	29,99	74		ENE	1	Cloudy.
	2	0	51,5	60,5	29,96	75		E	1	Fine.
23	7	0	45	58,5	29,94	78		E	1	Foggy.
	2	0	50,5	61	29,93	70		E	1	Fine.
24	7	0	45	58	30,10	73		E	1	Cloudy.
	2	0	47	59	30,14	71		E	1	Cloudy.
25	7	0	45	58	30,30	74		E	1	Cloudy.
	2	0	47	58,5	30,32	66		E	1	Cloudy.
26	7	0	44	57	30,33	70		E	1	Cloudy.
	2	0	46,5	58	30,31	70		E	1	Cloudy.
27	7	0	43	57	30,25	75		NE	1	Fair.
	2	0	48	57,5	30,24	73		NNE	1	Cloudy.
28	7	0	45	57	30,30	78		NE	1	Foggy.
	2	0	45	57	30,31	72		ENE	1	Cloudy.
29	7	0	42	56,5	30,36	71		E	1	Cloudy.
	2	0	44	56,5	30,35	67		NE	1	Cloudy.
30	7	0	42	55	30,36	75		NE	1	Cloudy.
	2	0	44	56	30,36	71		NE	1	Cloudy.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Dec. 1	8	0	42	52,5	30,38	76	0,035	E	1	Cloudy.
	2	0	44	56	30,35	74		NE	1	Cloudy.
2	8	0	42	55	30,26	75	0,043	NE	1	Cloudy.
	2	0	44	55	30,19	70		NE	2	Cloudy.
3	8	0	39	54	30,19	75		NE	1	Cloudy.
	2	0	41	54	30,26	65		E	1	Cloudy.
4	8	0	32	53	30,29	63		WSW	1	Cloudy.
	2	0	38	54	30,25	60		SW	1	Cloudy.
5	8	0	32	52	30,28	75		WSW	1	Cloudy.
	2	0	40	52	30,32	75		WNW	1	Cloudy.
6	8	0	38	52	30,30	74		SW	1	Cloudy.
	2	0	43	53,5	30,22	72		WNW	1	Cloudy.
7	8	0	31	51	30,09	73		WNW	1	Fair.
	2	0	38	53	29,99	71		SW	1	Foggy.
8	8	0	39	51	29,90	73		SW	1	Cloudy.
	2	0	44	53,5	29,87	71		SW	1	Cloudy.
9	8	0	42	51	29,77	76		SSW	1	Cloudy.
	2	0	45	53	29,67	70		SSW	1	Cloudy.
10	8	0	43	52	29,51	74		E	1	Cloudy.
	2	0	50	55,5	29,38	70		SE	1	Fine.
11	8	0	47	52,5	28,72	*		ESE	2	Rain.
	2	0	53	56	28,77			SW	2	Cloudy.
12	8	0	50	55	29,15		0,130	SW	1	Cloudy.
	2	0	53	57	29,28			SSW	1	Cloudy.
13	8	0	54	57	29,20		0,140	S	2	Rain.
	2	0	53	60	29,28			S	2	Fair.
14	8	0	48	57	29,47		0,060	S	2	Cloudy.
	2	0	53	59	29,41			SE	2	Cloudy.
15	8	0	45	57,5	29,48			SSW	1	Cloudy.
	2	0	50	58,5	29,50			SSW	1	Cloudy.
16	8	0	50	57	29,15		0,704	SW	1	Rain.
	2	0	49	58,5	29,30			NW	1	Cloudy.

* This interruption, in the observations of the hygrometer, was occasioned by one end of the whalebone having slipped out of the pincers by which it is held.

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1793	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Dec. 17	8	0	39	56	29,84		0,158	SW	1	Fine.
	2	0	48	58	29,79			S	2	Rain.
18	8	0	51	56,5	29,56		0,251	SSW	2	Cloudy.
	2	0	51,5	59,5	29,48			SSW	2	Cloudy.
19	8	0	43	57	29,44		0,160	S	2	Fine.
	2	0	51	59	29,49			S	2	Fine.
20	8	0	37	57	29,58			SSE	1	Cloudy.
	2	0	40	58	29,57				1	Foggy.
21	8	0	38	56	29,49			SE	1	Fine.
	2	0	44	57,5	29,29			SE	2	Cloudy.
22	8	0	42	55	28,97		0,074	E	1	Cloudy.
	2	0	47	57	28,99			ESE	2	Cloudy.
23	8	0	44	55,5	29,28		0,054	E	1	Cloudy.
	2	0	49	59	29,44			E	1	Cloudy.
24	8	0	43	55,5	29,72			NE	1	Cloudy.
	2	0	46	58	29,78			NE	1	Cloudy.
25	8	0	42	55	29,96			NE	1	Cloudy.
	2	0	43	56,5	29,98			NE	1	Cloudy.
26	8	0	41	55	30,11			E	1	Cloudy.
	2	0	41	56	30,10			E	1	Cloudy.
27	8	0	38	54	30,03			E	1	Cloudy.
	2	0	40	56	29,98			E	1	Cloudy.
28	8	0	37	53	29,94			NE	1	Cloudy.
	2	0	39	54	29,89			NE	1	Cloudy.
29	8	0	36	52	30,05			NNE	1	Cloudy.
	2	0	39	54	30,11			NNE	1	Fine.
30	8	0	30	50	30,14			E	1	Cloudy.
	2	0	32	52	30,09			ESE	1	Foggy.
31	8	0	38	50	29,80			S	2	Cloudy.
	2	0	39	54	29,74			SE	2	Fair.

1793.	Thermometer without.			Thermometer within.			Barometer.			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.				Inches.
January	48	28	37,6	54	45	49,6	30,52	28,98	30,16				1,565
February	52	30	42	57	49,5	52,5	30,22	29,29	29,80				1,581
March	52	33	41,5	58	49,5	54	30,21	29,07	29,84	83	51	64	1,162
April	60	33	45,5	59,5	50	55	30,27	29,22	29,89	85	46	61,3	1,095
May	69	44	54,4	64	56	59,2	30,29	29,24	30,04	70	43	57,4	0,865
June	72	47	59,1	63	57	60,6	30,20	29,68	29,96	72	46	56,3	0,427
July	89	54	68	76	62	69	30,30	29,74	30,03	74	43	55,4	1,616
August	80	52	63,7	70,5	63,5	66,3	30,28	29,34	29,95	73	47	58,4	1,315
September	66,5	42	55,4	66	56	61,2	30,45	29,41	29,98	82	50	66	2,452
October	65	35,5	54,3	64	56	60,6	30,48	29,22	29,98	84	57	68,5	1,137
November	56	31	45,5	61	53	56,4	30,36	29,05	29,80	83	64	72,2	2,104
December	53	30	42,9	60	50	55,1	30,38	28,72	29,74				1,809
Whole year			50,8			58,3			29,93				17,128